



DEPARTMENT OF THE AIR FORCE
AIR COMMAND AND STAFF COLLEGE (ATC)
MAXWELL AIR FORCE BASE, ALABAMA 36112

File?

13 MAY 1982

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Assistant National Intelligence Officer
for Strategic Programs
Central Intelligence Agency
Washington, D. C. 20505

Dear [redacted]

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I am most pleased to invite you to address selected members of the Air Command and Staff College (ACSC) Class of 1982 in the "Intelligence and National Security" Tailored Instructional Program (TIP) on 1 June 1982 at 0830. A two-hour period has been reserved for you to discuss Strategic Intelligence Estimates. Please use this time as you deem appropriate--formal lecture, informal remarks, question and answer, or any combination thereof.

The TIP is an elective course designed for ACSC course officers with follow-on assignments to national or Department of Defense intelligence organizations or to operations, plans, programs, or R&D staff positions which involve the use of national intelligence products. It is intended to provide an introduction to the history, structure, and operation of the US Intelligence Community and the production and use of intelligence products. Your wealth of knowledge and experience will provide the course officers a valuable insight into the intelligence activities of this country.

Lieutenant Colonel Robert E. Setlow, telephone (205) 293-6519 will coordinate the details of your visit.

I greatly appreciate your support of our educational program and look forward to having you as our guest next month.

Sincerely

ROBERT W. KLINE
Colonel, USAF
Vice Commandant

STRATEGIC INTELLIGENCE ASSESSMENTS

W70014-1L/1D

OBJECTIVES:

1. Comprehend the major issues involved in the production of intelligence estimates on Soviet strategic forces and objectives.
 - 1.1 Summarize the historical record of U.S. Intelligence estimates on Soviet strategic forces.
 - 1.2 Explain the analytical methodologies employed in estimating the present and future capabilities of Soviet strategic forces.
 - 1.3 Give examples of the major problems encountered in producing estimates on Soviet forces and objectives.

OVERVIEW:

Estimative intelligence on Soviet strategic forces and objectives constitutes one of the principal production functions of the U.S. Intelligence Community. As we develop and deploy our own strategic forces, it is important that military planners and defense policymakers understand present and future Soviet strategic capabilities and comprehend likely Soviet responses to our policies and actions. The arms control negotiations of the past decade have given impetus to the efforts of the Intelligence Community to improve the quality of its estimates.

Planners and policymakers must, however, bear in mind the limitations of estimative intelligence, as Klaus Knorr points out in one of the later readings:

Although the future, within some framework of particulars, can be estimated, it cannot, of course, be known. To estimate is to guess in order to reduce uncertainty dictated by lack of knowledge. The assumptions and preconceptions about reality that structure the guesswork can be more or less rigorously deduced from past behavior. But--as the historical record discloses....even the most sophisticated assumptions can lead threat perception astray. To depend wholly on any one preconception or set of assumptions is to court surprise. This risk is magnified by the tendency that the selection of an assumption about the real world becomes an act of cognitive closure that easily leads the perceiver to be close-minded and to ignore or explain away

discrepant information. It must therefore be accepted that although good estimates can reduce uncertainty about the future, even the best cannot be depended on to prove it.

Knorr also identifies several kinds of predispositions that intervene to affect the selection of assumptions and receptivity to incoming information and which are apt to distort estimates. Among these are emotions, strong ideological commitments, bureaucratic behavior, and wishful thinking.

Bear these considerations in mind while you review the controversy over U.S. intelligence estimates of Soviet strategic forces in the readings. We will have an opportunity to explore them more deeply during the lecture on weapon system analysis as applied to Soviet strategic estimates. Two of the historically controversial subject areas of these estimates--Soviet strategic bombers and ICBM's--will be highlighted to illustrate the techniques of weapon system analysis.

READINGS:

Assigned:

"Debate Over U.S. Strategic Forecasts: A Mixed Record"

"The National Intelligence Estimates A-B Team Episode
Concerning Soviet Strategic Capability and Objectives"



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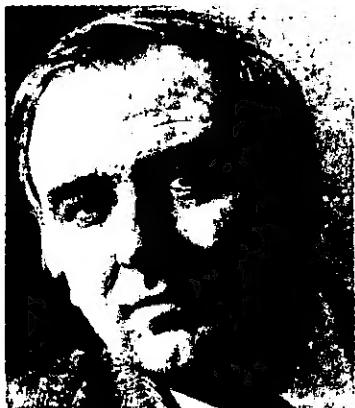
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NO. 3

DEBATE OVER U.S. STRATEGIC FORECASTS: A MIXED RECORD

LES ASPIN



THE AUTHOR: Congressman Aspin is Chairman of the Oversight Subcommittee of the House Permanent Select Committee on Intelligence and serves on the House Armed Services Committee and the Government Operations Committee. He was first elected to Congress in 1970. Aspin served in the U.S. Army from 1966 to 1968 as an economic adviser in the office of the Secretary of Defense. He is a graduate of Yale University, received a Master's degree from Oxford University and a Ph.D. in economics from the Massachusetts Institute of Technology.

IN BRIEF

The charge has resounded in recent times that the United States intelligence community has chronically and woefully underestimated both the pace and magnitude of the Soviet strategic build-up. Yet, an analysis of the available record of forecasts with respect to eight major Soviet weapons developments—extending from the first Soviet A-bomb explosion in 1949 to the improvements in Soviet ICBM accuracy and yields in the 1970s—shows that the performance has been mixed, consisting of overestimates as well as underestimates, and in at least two instances of predictions that were on or close to the target. Few of the mistakes that have been committed in forecasting can be attributed to errors in intelligence gathering; most of them have been the function of more-or-less inevitable human foibles. With the demise of SALT, estimates of future Soviet strategic programs are apt to be wider off the mark than they would have been under a SALT II Treaty, because the reference points provided by the Treaty for U.S. intelligence have been removed, and precisely because the human element in intelligence evaluation and forecasting is thus again maximized.

A related article: DEBATE OVER U.S. STRATEGIC FORECASTS: A POOR RECORD
by WILLIAM T. LEE appears on Page 14 of this issue. A REBUTTAL by
CONGRESSMAN ASPIN appears on Page 25.

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"It is . . . a matter of record that the growth of the Soviet ICBM force was underestimated for a decade after the 'missile gap' by the entire intelligence community—including Pentagon 'hawks.'"

Lt. Gen. Daniel O. Graham, USA (Ret.)

"But the history of the past twenty years shows quite the reverse. Few indeed are the instances when the Soviet military threat later turned out to be greater than the estimated 'worst case.' Usually, the government's experts overestimated the danger."

George B. Kistiakowsky

The death of SALT II turns the focus of U.S. strategic intelligence away from "verification" and back to the old business of "forecasting." SALT provided for some degrees of restraint and certainty: We knew how far the Soviets were allowed to go, and the task was to verify their compliance with these restrictions. Without SALT, there are no limits or guidelines. The United States must rely purely on its skills in strategic forecasting—in projecting the future, including future Soviet strategic intentions and capabilities.

The first forecast since the deferral of SALT II has been completed and leaked to the press. The new National Intelligence Estimate—NIE 1138-79—reportedly indicates that without SALT II the Soviets could amass about 14,000 highly accurate ICBM warheads by the late 1980s. By contrast, an extension of SALT II beyond its 1985 expiration date would allow the Soviets only about 6,000 such warheads; if SALT II were in effect, therefore, the presumption would be that the Soviets would build up only to that limit.¹ U.S. strategic force planning would be based on this assumption and U.S. intelligence agencies would be concentrating on verifying Soviet compliance. Now, without SALT II, all we have to go on is this new intelligence estimate. Who knows whether it has validity or not? If U.S. policymakers do believe it to be valid, however, then they will have to think about a requisite expansion of U.S. strategic nuclear forces. Tens of billions of dollars potentially ride on a decision of whether or not to trust this intelligence estimate.²

How good is U.S. intelligence at this task of strategic forecasting? As the passages quoted above indicate, this question is highly controversial.³ Over the years, many analysts, particularly those in arms control circles, have contended that we have consistently overestimated Soviet strategic capabilities. More recently, other analysts, not generally associated with arms control, have argued that we have in fact consistently underestimated Soviet strength.

This controversy can, to some extent, be resolved by examining the record. Considering

the salient developments in the history of the nuclear arms competition, we can ask if the U.S. intelligence community has been right or wrong in its forecasts—and if wrong, in which direction (too high or too low) it has erred and for what reasons. The key developments have been:

1. The first Soviet explosion of an atomic bomb, 1949.
2. The first Soviet explosion of a hydrogen bomb, 1953.
3. The "bomber gap," 1955-1958.
4. The "missile gap," 1958-1961.
5. Soviet deployment of an anti-ballistic missile (ABM) system, 1962 onward.
6. Soviet deployment of missiles with multiple independently targetable reentry vehicles (MIRVs), 1965-1974.
7. Soviet intercontinental ballistic missile (ICBM) and submarine-launched ballistic missile (SLBM) deployments, 1962-1969.
8. The rate of improvements in Soviet ICBM accuracy and yield, 1969 onward.

Such an analysis should provide us with some idea of how well U.S. intelligence will be able to estimate future Soviet defense capabilities in the absence of SALT.

The A-Bomb and the H-Bomb

When the Soviets exploded their first atomic bomb in August 1949, the United States had very little information about Soviet nuclear research. Before the detonation, General Leslie Groves, wartime director of the Manhattan Project, predicted that America's atomic monopoly would last twenty years. Scientists involved in the project, on the other hand, believed in 1945 that the Soviets would duplicate the U.S. achievement within five years. The scientists' expectation encouraged the Atomic Energy Commission to establish, through the Air Force, a program for airborne collection of radioactive particles in the atmosphere, which would detect the explosion of any atomic device anywhere in the world. The program began operations in 1948 (and continues to this day).

As the end of the decade approached and no Soviet A-bomb materialized, the year of anticipated danger, from the vantage point of the U.S. intelligence community, receded progressively. Just before the Soviets actually detonated an atomic device in 1949, they were officially expected to do so in 1952 at the earliest.⁴ The hydrogen bomb, set off by the Soviets in 1953, came as less of a surprise: the United States had predicted that the Soviets would achieve that milestone by 1954.

Why did General Groves underestimate, the scientists correctly estimate, and later most

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analysts underestimate again how soon the Soviets would explode an A-bomb? And why was the H-bomb prediction so close to the mark?

The problem was not one of optimism about Soviet intentions. Indeed, in the first five years after the war, official circles in Washington generally held to the alarmist image of a Soviet Union bent on constant, implacable expansionism. Rather, the intelligence error on the A-bomb hinged on habit and personal intuition. General Groves thought that the Soviets would take twenty years to build the bomb because, like President Truman, he simply did not believe that "those Asiatic" Russians,⁸ valiant though they might be in standing up to the Germans on the battlefield, had the technological talents to duplicate what his scientists at Los Alamos accomplished in four years. The scientists' prediction that the Soviets would have a bomb within four or five years was modeled on their own experience. That is how long it had taken them to build the bomb: It was a fairly straightforward exercise in physics and engineering, of which they deemed their Soviet counterparts quite capable. In the end, intelligence analysts underestimated the development pace for the Soviets because of what the atomic scientist, Isadore Rabi, characterized as a "peculiar kind of psychology": after the initial estimate in 1945 that the Soviets could get a bomb in four or five years, "every year that went by, you kept on saying 'five years.'"

The close prediction of the Soviet Union's H-bomb detonation in 1953 was purely a matter of chance—a very good guess and little more. The principle of radiation pressure, the essence of the H-bomb, was not even demonstrated in the United States until 1951. Indeed, some officials believed the Soviets could get an H-bomb before 1953. In an attempt to encourage President Truman to forge ahead with the American H-bomb project in 1950, General Loper of the AEC's Military Liaison Committee argued in a memorandum to the President that available intelligence (almost nonexistent) was consistent with the theory that the Soviets already had the hydrogen bomb.⁹

The Bomber Gap

In 1955, Air Force Intelligence predicted that the Soviets would field a force of 600 to 700 long-range bombers by 1959. The National Intelligence Estimate (NIE) for that year was slightly more modest, predicting about 500 bombers by mid-1960. As it turned out, by mid-1961 the Soviets had deployed only 190 long-range bombers.¹⁰

Estimates of bombers grew out of a projection made in 1950—incorporated in a milestone Cold War document called NSC-68—that the Soviets would possess a stockpile of 200 atomic

bombs by 1954.⁸ This projection was based, in part, on the rate at which the United States had been able to build bombs. Given this projection and NSC-68's explicit assumption that the Kremlin was bent on expansion and that the United States was the Soviet Union's principal enemy, intelligence agencies naturally began thinking about how the Soviet Union would deliver the bombs to U.S. territory.

In 1954, Western attachés in Moscow observed a new Soviet long-range bomber flying overhead at the May Day military parade. On the basis of this report, U.S. intelligence made some assumptions about when the Soviets had begun development of this bomber and how quickly they might be able to deploy it in significant numbers. A study concluded that the bomber's design had been completed in 1952 and its first prototype flight made in 1953. In accordance with U.S. experience, it was estimated that mass production could not begin before 1956, and a substantial force could not be deployed before 1960.⁹

The next May Day parade, in 1955, rudely upset these calculations, or at least appeared to do so. Although the aviation part of the parade was canceled, Western observers reported seeing as many as twenty of the long-range bombers in the air during parade rehearsals. Intelligence now updated its earliest estimates. The design of the plane was assumed to have been completed two years earlier than the original finding, and mass production to have begun in 1954. If the Soviets could produce twenty aircraft per month over the next three years, then a force of 700 aircraft by 1959 was plausible.¹⁰

Yet, in 1956 and 1957, U-2 flights produced hard evidence that Soviet production rates fell far below the pace that had been estimated by U.S. intelligence two years earlier. Two factors were involved in this error: an intelligence mistake and a misunderstanding of Soviet strategic intentions.

First, unbeknown to the Western attachés, the Soviets were flying the same bombers back and forth in the 1955 parade preparations; the attachés mistakenly counted each overflight as a separate bomber.¹¹ Second, the United States, believing that its own territory was the ultimate target of the Soviet Union's nuclear ambitions, naturally assumed that the Soviets would produce intercontinental bombers at the fastest rate possible. However, the Soviets apparently decided that the principal threat to the Soviet Union lay around the periphery of the Soviet landmass, whence Russia had historically been threatened and where the United States happened to be stationing its own nuclear strike forces. Thus, the Soviets used most of their production capacity to build medium-range bombers rather than a long-range force.¹²

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The Missile Gap

The Soviet Union launched its first orbital satellite in October 1957.¹³ Although the CIA had foreseen this development years in advance, the actual launching triggered fears that the United States would soon be vulnerable to an ICBM attack. *Sputnik*: the very word evoked a nightmare vision of the Soviets outpacing the Americans in missile technology. Khrushchev exploited this American fear by publicly making outrageous statements about the capabilities of Soviet missiles which he knew at the time—and we know only in retrospect—to be false.

Air Force Intelligence warned in a November 1957 NIE that the Soviets could deploy 500 ICBMs by the middle of 1960 and 1,000 by 1961. The CIA believed a more reasonable estimate to be 100 ICBMs by 1960 and 500 by 1961. The wide difference in the two estimates hinged on conflicting views of when the Soviets would be able to begin mass production of their first ICBM, the SS-6. A halt in the Soviet test program, in April 1958, was interpreted by the Air Force as an indication that the missile was ready for deployment, whereas the CIA saw it as evidence that technical difficulties were being experienced in the missile's development. Renewed test launches in 1959 proved the CIA correct.

An entirely separate issue, however, was how many missiles the Soviets would produce each year. Apparently the Air Force picked 500 and the CIA 100 because they were round numbers. Since no one at that time knew the location of Soviet missile manufacturing plants, neither an actual count nor an inference from industrial volume was possible.

Nor did anyone know what a Soviet ICBM emplacement would look like. The Air Force anticipated camouflaged sites, whereas the CIA argued that the deployment sites would resemble the missile test launchers at Tyuratam. Repeated U-2 flights over Soviet railway lines could not locate any deployed ICBMs, although Air Force Intelligence suspected various buildings to be camouflaged structures hiding missiles. Among these were a Crimean War memorial and a medieval tower. A U.S. photo-reconnaissance satellite took the first clear pictures of a Soviet ICBM site at Plesetsk in August 1960—laid out, as the CIA had predicted, just like the site at Tyuratam. According to the early Air Force projection, the Soviets should have deployed more than 500 ICBMs by this time, but satellite coverage detected no similar sites anywhere else.

The identification of an operational SS-6 site reopened the issue of how quickly the Soviets could produce the missiles. From studies of the Soviet economy and the cost of American ICBMs, the CIA assumed that the Soviets could start off producing ICBMs either on an "orderly"

schedule of three per month or on a "crash" program of fifteen per month. Assuming that the Soviets had been producing missiles since 1959, when their test program ended, the CIA calculated that under the orderly schedule the USSR would have 36 operational SS-6s by November 1960, and that they might accelerate production to reach 100 by mid-1961 and 450 by mid-1962. The Air Force, meanwhile, stuck to its original prediction of 500 missiles per year.

The Army and Navy intelligence organizations, whose client services carried on weapons programs that competed with Air Force missiles, pointed out technical deficiencies in the SS-6 tests and expressed doubt that the USSR would ever deploy "more than a few" of these missiles. In August 1961, another successful recovery of satellite film proved them right. Indeed, the Soviets had deployed no more than ten SS-6 missiles, all at Plesetsk. This discovery ended the "missile gap" for good.

The Soviets did have a substantial missile build-up in process—but it was in intermediate-range and not intercontinental missiles. The U.S. intelligence error, again, was one of mistaking Soviet priorities. Between 1958 and 1965, the Soviets deployed about 700 medium- and intermediate-range ballistic missiles (M/IRBMs) aimed at Western Europe. This comes to about 100 missiles per year—a figure between the CIA's "orderly" and "crash" estimates of 36 and 180 missiles per year respectively, but far short of the Air Force estimate of 500 per year.

Anti-Ballistic Missiles

Throughout the 1960s, intelligence analysts repeatedly predicted that the Soviets would deploy a nationwide anti-ballistic missile (ABM) system.¹⁴ In the early 1960s, the intelligence community estimated that the Soviets would deploy some 2,000 exo-atmospheric and 8,000 endo-atmospheric interceptors.¹⁵ In 1963–1964, the NIE on strategic defensive forces predicted that before 1975 the Moscow ABM system, just coming under construction, would be expanded to cover every major city with 500 to 1,500 interceptors. Furthermore, between 1964 and 1966, Pentagon analysts suspected that the Tallinn air-defense system would eventually serve as a nationwide ABM and managed to insert this speculation into some NIEs.

After 1967, construction of the Moscow ABM System seemed to halt with only 64 interceptors fielded. Those Tallinn sites were later proved to be for defense against high-altitude bombers. At this point, analysts in the Defense Intelligence Agency (DIA) and John Foster, then the Director of Defense Research and Engineering, speculated that the Tallinn sites could quickly be "upgraded" to a dual purpose SAM/ABM system. Further analysis, however, revealed

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that many of the Tallinn sites were badly located for ICBM interception, and that they lacked the nuclear warhead storage space essential for a workable ABM system.

Why was U.S. intelligence so eager to detect a Soviet ABM system that never did materialize? Part of this misjudgment was founded on an assessment of Soviet strategic doctrine. The Soviets were greatly concerned about strategic defense. They had an extensive air defense network to intercept bombers, and they had something of a civil defense program. Many intelligence analysts logically concluded that they would construct a comprehensive ABM system as well.

The type of Soviet ABM for which these analysts looked—a combination of exo- and endo-atmospheric interceptors—reflected American concepts of ABM design, which eventually were realized in the Spartan and Sprint missiles. The Spartan was a comparatively slow missile intended to intercept approaching missiles at or near the peak of their trajectories, when they would be moving at their slowest speed. The fast Sprint would be launched to home in on any reentry vehicles the Spartan might miss. Sprint involved an extremely close radar tracking. Perhaps because the endo-atmospheric approach was so demanding, however, the Soviets chose a different route altogether: an interceptor that would operate at medium altitude (200,000–500,000 feet). From this model, the Soviets developed the Galosh and Griffon interceptors, which used many of the same components.

Galosh was, and is, an ABM. Sixty-four of the interceptors remain deployed around Moscow. However, the Galosh radars use a mechanical means of tracking ICBM warheads, an extremely difficult technique. By 1967, U.S. intelligence analysts began to raise doubts whether the Soviets would ever make further investments in so ineffective a system.

Griffon is the missile deployed in the Tallinn system, now known as the SA-5 surface-to-air missile (SAM). NIE judgments with respect to Griffon's mission wavered from year to year. The Tallinn sites were successors to a system which the Soviets began building around Leningrad in the early 1960s and which the 1963 NIE deemed an "apparent" ABM ("apparent" had been formally established as a codeword in NIEs to indicate that the analysts had little confidence in the given judgment). In 1964, the CIA concluded that Griffon must be an anti-aircraft missile, primarily because its performance was so inferior to Galosh.

On the other hand, Soviet public statements were attributing ABM capabilities to Griffon; Khrushchev said it could hit "a fly in outer space." The Air Force, Army and the DIA were convinced that the CIA was grossly underestimating Griffon's capabilities. The 1965 NIE consequently noted that the intended mission

of the Tallinn sites was uncertain, a judgment repeated in 1966.

In bureaucratic terms, Pentagon intelligence analysts had large stakes invested in a Soviet ABM. The Joint Chiefs of Staff, whom DIA represented within the intelligence community, and the Air Force needed the specter of a Soviet ABM as a rationale for developing MIRVs (multiple warheads) for U.S. missiles. The Army needed a finding that Soviet ABMs were effective in order to overcome the many doubts about its own ABM program. The analysts could (and did) cite Soviet documents—including classified ones supplied by that premier spy, Penkovskii—to show that the Soviets wanted to build an ABM. They conveniently ignored Soviet documents written after 1965, which expressed grave doubts about the feasibility of ABMs. Those documents had not been obtained clandestinely and as such were dismissed as deliberate Soviet misinformation.

Intelligence analysts were also misled by an assumption about the Soviet military as an eminently rational, far-sighted institution. Many thought the Tallinn system *must* be an ABM—rather than the high-altitude anti-bomber system it was—because it could have no other rational purpose. By that time U.S. bombers were simulating penetrations of Soviet air space at *low* altitude. If Tallinn were a high-altitude system, then the Soviets were building weapons for which there was no mission—an idea thoroughly unpalatable to those who viewed Soviet defense programs as undisturbed by bureaucratic impulses, quirks or mistakes.

The CIA eventually adopted the view that as late as 1967 or 1968 the Soviets still intended to deploy Galosh nationwide, but that improvements in American strategic forces—particularly the MIRV system—convinced them that they needed to go back to the drawing board. This highly doubtful argument salvaged the institutional self-esteem of the Air Force and DIA by validating their argument that the Soviets intended the Galosh and Griffon to be nationwide ABMs, while conceding to the CIA the accuracy of its contention that the Soviets were not deploying an effective ABM system.

Soviet MIRVs

The prospect that the Soviets might place multiple independently targetable warheads (MIRVs) on their ICBMs was first mentioned in the 1965 NIE. The NIE stated it would take four or five years for the Soviets to develop and begin deploying MIRVs that were sufficiently accurate for the destruction of "hardened" (i.e., blast-resistant) targets such as the newly developed Minuteman ICBM silo. At the time, there was no evidence that any Soviet MIRV program had even begun. Thus the earliest date for Soviet MIRV deployment, inferred from the

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1965 NIE, was 1969. In 1966 and 1967, Soviet space shots demonstrated some of the technology necessary for MIRVing. As a result, the Air Force insisted that the NIE contain a judgment that the Soviets were in fact developing a MIRV.¹⁶

In August 1968, the United States observed the first test of the SS-9 "triplet," the three-warhead ICBM. The SS-9 was a very large missile. It was believed that such a missile would be ideally suited to the task of digging out Minuteman silos. However, even the highest estimates of ultimate SS-9 deployments—the Air Force's projection of 700—envisioned a number that was insufficient to destroy 1,000 Minuteman missiles. Thus, analysts who believed that the Soviets were intent upon capabilities to knock out Minuteman reasoned that the Soviets must be planning to place multiple warheads on the SS-9. The triplet tests seemed to confirm this suspicion.

The issue then became whether the triplet was a MIRV or merely an unsophisticated MRV—i.e., whether each of the three warheads could be aimed at a separate target, or whether all three must be aimed at the same general area. Each warhead of the triplet was placed on a rail in the nose-cone of the SS-9. The rails did not rotate to allow repositioning and retargeting of the warheads. This feature convinced CIA analysts that the SS-9 was simply an MRV. Therefore, the 1968 NIE did not expect a Soviet MIRV until 1978—the end of the period covered by the estimate.

However, analysts outside the intelligence community, most notably in the Pentagon's Directorate of Defense Research and Engineering, noted that the timing of the warhead releases from the SS-9 could cause the warheads to fall in various triangular patterns. They concluded from the pattern of releases during test-flights in the Pacific that the Soviets were indeed adapting these "triangles" (or "footprints") to match the configuration of U.S. missile silos. A missile force of 400 to 700 SS-9s, each with three warheads that could be aimed at three silos, might be very effective against Minuteman after all.

The triplet issue took on all the more importance because the Nixon Administration was seeking Congressional approval of the Safeguard ABM system designed to protect Minuteman against Soviet attack. If the SS-9 lacked MIRV capability, then Minuteman needed no protection; if the triangular pattern of the triplet coincided with the distance between three U.S. Minuteman silos, however, then the case for Minuteman vulnerability might still be valid. Furthermore, Henry Kissinger wanted the ABM as a "bargaining chip" in the SALT I negotiations that were just getting underway. Consequently, Kissinger summoned the CIA estimators and the Pentagon DDR&E analysts to the White House for a series of special meet-

ings. From these sessions, Kissinger concluded that the triplet was indeed a primitive MIRV, and he instructed the CIA to rewrite the 1969 NIE to include more evidence supporting both sides of the controversy.¹⁷ (During 1969, therefore, two NIEs on Soviet strategic forces were disseminated: one at the beginning of 1969, which had been prepared the previous year, and one in the fall of 1969 at the new Administration's request.)

In an important sense, the whole argument was artificial. In fact, the Soviets had several programs in parallel: not just an effort to test a primitive MIRV for the SS-9, but also a program to design more sophisticated MIRVs for the next generation of ICBMs. The United States knew nothing about this next generation. Judging by the U.S. decision to stop its own ICBM programs with the third-generation Minuteman, intelligence estimators may have believed that the Soviets would not proceed beyond the SS-9.

In any event, the first 1969 NIE took a wholly different approach to the issue of when the Soviets would be able to deploy a true MIRV. The estimators postulated two possible Soviet approaches: low force/low technology and high force/high technology. The former was based on the assumption that the Soviets would deploy the triplet, not attaining a true MIRV until 1974. The latter assumed the Soviets would skip the triplet and move directly to a MIRV for the SS-9. It was believed that the Soviets, using the technology tested in the space launches of 1966–1967, might be able to begin deploying MIRVs as early as 1971.¹⁸

As it happened, the first Soviet MIRV was deployed on an entirely new, fourth-generation ICBM in 1975. The Soviets never tried to build a truly MIRVed SS-9. But the intelligence estimates went through various phases. First they overestimated (in 1965 the estimate was 1970), then underestimated (in 1968 the prediction was 1978), then overestimated again (in 1969 the projection was 1971). The varying estimates reflected the different political needs of successive U.S. administrations, as well as a rather vacuous argument over which U.S. terminology (MIRV or MRV) was a more appropriate description of the SS-9 triplet.

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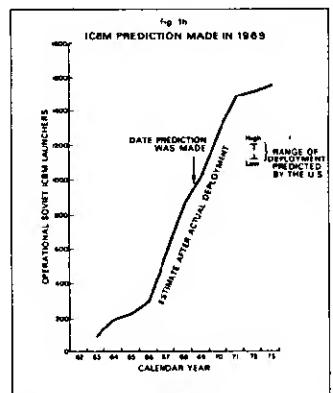
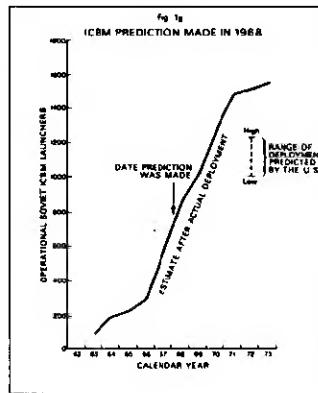
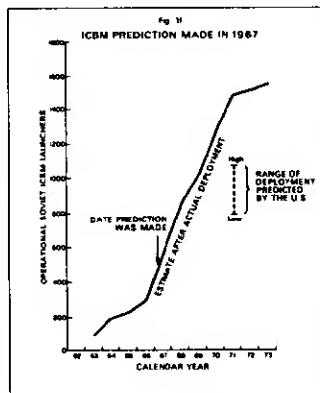
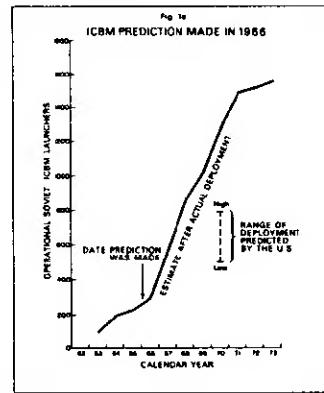
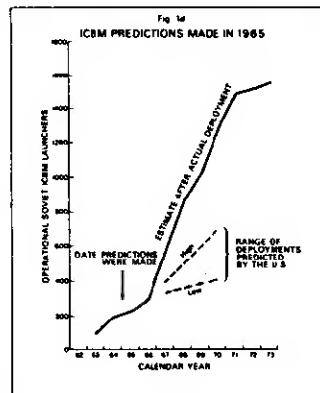
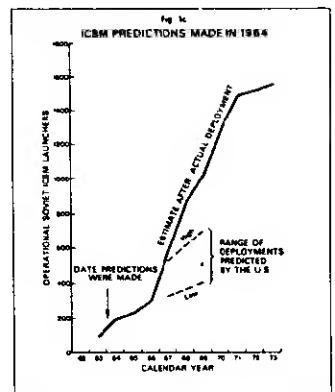
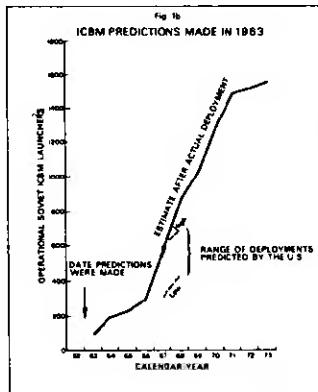
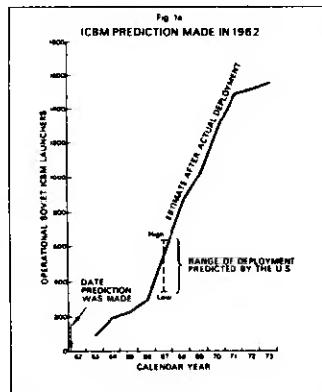
The Soviet ICBM Force Size

In a series of articles in 1974, the prominent strategic analyst, Albert Wohlstetter, argued that the NIEs between 1962 and 1969 consistently *underestimated* future Soviet strategic offensive capabilities.¹⁹ Wohlstetter's ostensible motive was to challenge the commonly accepted thesis that military intelligence invariably *overestimated* Soviet capabilities to justify its own costly defense programs.

Motives aside, Wohlstetter advanced the idea that these underestimates represented a sys-

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Wohlstetter Charts of U.S. Predictions of Soviet ICBMs, 1962-1969



tematic bias inside the CIA and the intelligence community as a whole—a bias against recognizing the grand scope of Soviet ambitions for ICBM procurement. As the charts on page 36 (reproduced from Wohlstetter's text) indicate, the intelligence agencies did underestimate the number of Soviet ICBM launchers in making projections of future Soviet capabilities. Moreover, as the Soviet build-up accelerated, intel-

ligence projections did not improve; in some cases they even worsened.

Why did this happen? One explanation is that of "mirror-imaging." After 1965, the CIA expected that the Soviets would place MIRVs on their ICBMs just as the United States had done. This expectation led intelligence analysts to project that the Soviets would deploy fewer

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ICBMs than they finally emplaced. The intelligence community based its estimates on the finding within the United States Defense Department that qualitative improvements to ICBMs were far cheaper ways to gain additional capability than quantitative increases in the force itself. In response to Wohlstetter's charge of underestimation, Lt. Gen. Daniel O. Graham, Director of the DIA, testified in 1975 that "the continuing evidence of qualitative improvement was a prime contributor to our underestimation of ICBM deployment . . . It seemed logical at the time that the Soviets would try to use their advantage in throw-weight by equipping their ICBMs with MIRVs which could . . . overwhelm the then-programmed U.S. ABM . . . and . . . permit multiple targeting [of U.S. ICBMs]. . . ."²⁰

The Soviets, however, decided instead to build larger numbers of ICBMs. Thus, the CIA did underestimate the number of missile launchers that the Soviets would construct—but it did not massively underestimate the offensive capabilities of the USSR as a whole.

Second, the CIA knew that resources in the USSR were scarce and believed that the major Soviet military investments were going into other Soviet military programs. The CIA during these years vastly overestimated the number of ABMs the Soviets would produce—and this, too, contributed to an underestimation of Soviet ICBM production. In 1962, when small numbers of Soviet ICBMs were predicted, the United States was also anticipating deployment of something like 10,000 ABM interceptors. Defense Secretary Robert S. McNamara suggested in his 1964 Posture Statement that ICBM programs would tend to constrain "large and very costly new programs such as an effective anti-ballistic missile defense system."²¹ When the intelligence community (incorrectly) concluded that the Soviets were about to deploy a massive ABM network, it was logically reasoned that the Soviets would not build a very large ICBM force. Indeed, the greatest ICBM underestimates, those for 1965 and 1966, coincide with the greatest ABM overestimates.

Third, the general underestimation of Soviet ICBMs included a whopping overestimation of one system in particular, the SS-9. In 1969, DIA projected 420 SS-9s; the Air Force expected as many as 700. In fact, the Soviets never deployed more than 280 and devoted most of their resources to constructing nearly 1,000 smaller SS-11 missiles.²² Had the Soviets gone ahead with SS-9s, the same resources would have purchased something closer to the number of SS-9s predicted by the intelligence community (except for Air Force Intelligence). Thus, in terms of projecting actual offensive capabilities, U.S. intelligence was not so far off the mark as Wohlstetter suggests. Still, the agencies did err in not foreseeing the new Soviet

emphasis on larger numbers of much smaller missiles, which greatly enhanced Soviet power to destroy American industrial and population targets.

Fourth, about 50 per cent of the intelligence community's underestimations, for each year in the late 1960s, is accounted for by the Soviet Union's decision not to retire about 200 obsolete SS-7 and SS-8 ICBMs, contrary to expectations of U.S. intelligence. Thus, when Wohlstetter's chart indicates an underestimate of about 400 ICBMs in 1967, roughly 200 of those were due to an expectation that the Soviets would retire older, more inaccurate missiles.

The lesson to be learned from a closer look at the Wohlstetter study is not, as is now popularly perceived, that the United States has consistently underestimated the offensive capabilities of Soviet missile forces—but rather that, as Wohlstetter himself avers, we underestimated some aspects of that force, overestimated other aspects and made some accurate predictions. Perhaps these cases of optimism and pessimism balanced out when the Defense Department attempted to base its own force planning on these intelligence projections. (For example, McNamara testified that the United States planned forces in the early 1960s under the assumption that the Soviets would mount an enormous ABM capability—a belief that probably more than compensated for the assumption that they would build a relatively small ICBM force.)²³

The intelligence errors on this score appear to be interconnected: low ICBM estimates were directly linked with high ABM estimates. In short, the error is caused not so much by simple counting mistakes as by a misjudgment of *how* the Soviets planned to allocate their defense resources.

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Soviet ICBM Accuracy and Yield

Estimating improvements in Soviet ICBM accuracy and explosive yield is today's critically important issue. It is the combination of these two factors that determines the vulnerability of the U.S. force of land-based ICBMs in fixed silos.²⁴

Throughout the 1960s, there was little official concern about the vulnerability of Minuteman. In 1968, Defense Secretary Clark Clifford wrote a memorandum to President Johnson, one paragraph of which pointed to the possibility that MIRV deployments of the SS-9 constituted a potential threat to the Minuteman force, and then suggested various solutions to the problem. The Joint Chiefs of Staff convinced Clifford to delete the paragraph.²⁵

The Nixon Administration took Minuteman vulnerability more seriously. If the Soviets could deploy a force of 700 SS-9s, each with triplet warheads (as U.S. intelligence was pro-

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jecting at the time), they could hypothetically aim two warheads at each of the 1,000 Minuteman silos, thereby ensuring the destruction of nearly all of them. The Office of the Secretary of Defense believed at the time that the Soviets could achieve accuracies of .25 nautical miles CEP (meaning half the warheads would strike within .25 miles of the target point) with the SS-9 triplet by 1974-1975. It calculated that this accuracy, combined with each warhead's estimated 5-megaton yield, would permit the Soviets to knock out 95 per cent of the Minuteman force in a first strike.²⁶

The CIA disagreed. CIA weapons analysts did not believe the "triplet" technology could be improved sufficiently to attain the postulated .25 nautical-mile CEP. The SS-9 triplet had failed to demonstrate accuracy better than .5 nautical miles—not nearly enough, even given the high yields of the Soviet warheads, to destroy missile silos with high probability. In September 1969 the Board of National Estimates therefore drafted a paragraph to the effect that the Soviets could not, and would not try to, achieve a first-strike capability against Minuteman during the 1970s.

However, Secretary of Defense Melvin R. Laird had publicly raised, in open Congressional testimony, the SS-9's threat to Minuteman. Reportedly, Laird's special assistant, William Baroody, went to Central Intelligence Director Richard Helms and asked him to delete the contrary paragraph from the 1969 NIE. Helms complied. When questioned by Senator Frank Church's Senate Intelligence Committee about this matter, Baroody testified that he could not remember "specifically bring[ing] pressures to bear on the Director of Central Intelligence to delete or change any particular paragraph." However, Abbot Smith, then the chairman of the CIA's Board of National Estimates, does recall the episode as the only instance of direct political interference with the NIEs that he could remember in his twenty years with the agency.²⁷

In April 1971, TRW, Inc. completed a study sponsored jointly by the CIA and DDR&E. It demonstrated that Soviet technology for the SS-9 could not achieve accuracies better than the .5 nautical-mile CEP estimated previously by the CIA—an error factor inadequate for an effective first strike against Minuteman.²⁸ By this time, however, the deployment of Safeguard ABM to defend Minuteman sites had already been authorized.

In 1973, early Soviet testing for fourth-generation ICBM programs (the SS-17, SS-18 and SS-19) reopened the controversy over Minuteman vulnerability. Initial press leaks suggested that the first tests showed accuracies for the new missiles to be little better than the .5 nautical-mile CEP of the older systems. Furthermore, since the new missiles carried more

warheads than the SS-9 and had similar or lighter throw-weight, the yields of each warhead would be less than the SS-9's. In short, the warheads would not threaten the Minuteman silos. In response to these reports, a Soviet officer reportedly told U.S. officials during the June 1974 Moscow summit that the United States was underestimating the accuracies of the new missiles. He claimed that .27 nautical-mile CEPs had already been achieved. U.S. analysts doubted this assertion.²⁹

Since that time, intelligence analysts have detected improved performance in Soviet missile accuracy, which—combined with relatively high yields—theoretically does pose a threat to the Minuteman missiles.³⁰ In fact, accuracy cannot be precisely estimated. In June 1979, Walter Pincus reported in the *Washington Post* that the accuracy of Soviet ICBMs was somewhat better than U.S. intelligence had predicted for that time.³¹ However, he also reported that the SS-18 and SS-19 warheads were now judged to have substantially lower yields than had once been projected for them: the analysts had reduced their estimate from 1.5 megatons to about 600 kilotons. The new pessimism in accuracy estimates and the new optimism in yield estimates virtually canceled each other out. This indicates the hazards, and also the importance, of casting precise estimates. (Had the CIA reduced the yield estimates without also accounting for improved accuracy, the perceived vulnerability of Minuteman for the mid-1980s would have dropped from 90 to 80 per cent—a perception that might have carried significant policy implications.)

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How Good Is Forecasting?

In sum, the record of U.S. intelligence in forecasting future Soviet strategic capabilities is a rather mixed one. Of the eight critical developments which we have examined (See Table 1), the intelligence community overestimated Soviet capabilities on three occasions, underestimated them once, and both over- and underestimated in two cases. The community was almost exactly on target once, and divided between accuracy and underestimation once. The one instance of unmitigated underestimation (in the prediction of the number of Soviet ICBMs) was linked to overestimates of other variables (especially deployment and numbers of Soviet MIRVs and ABMs). The one time when the prediction was nearly dead right (the timing of the first Soviet H-bomb) was a case of fortunate guesswork, based on no hard data.

The record of estimates on Soviet strategic forces bears out Albert Wohlstetter's conclusion: "Our officials sometimes overestimate, and sometimes underestimate, and sometimes even get it right. . . ."³² This mixed record is

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TABLE 1
U.S. Intelligence: Forecasts vs. Reality

Event	Date Prediction Made	Prediction	Actual	Over: + Under: — Right: 0
Date of Soviet A-Bomb	1945 (Groves)	1965	1949	—
	1945 (scientists)	1949		0
	1949 (intelligence)	1952		—
Date of Soviet H-Bomb	1950	1954	1953	0
Number of Soviet Long-Range Bombers By 1960 ("Bomber Gap")	1955 (Air Force)	600-700	190	+
	1955 (NIE)	500		+
Number of Soviet ICBMs By 1961 ("Missile Gap")	1957 (Air Force)	1,000	10	+
	1957 (CIA)	500		+
Number of Soviet ABMs	early 1960s	10,000	64	+
Date of Soviet MIRV Deployment	1965	1970	1975	+
	1968	1978		—
	1969	1971 or 1974		+
Number of Soviet ICBMs *				
By 1967	1964	325-525	570	—
	1965	330-395		—
By 1970	1965	410-700	1,299	—
	1966	505-795		—
By 1971	1967	805-1,080	1,513	—
	1968	1,020-1,251	1,527	—
	1968	1,158-1,276		—
ICBM Accuracy and Yield				
For SS-9 Accuracy	1969	.25 CEP **	.5 CEP	+
For New Missile Accuracy	1973	.5 CEP	.25 CEP	—
For SS-18/-19 Yield	1978	1.5 Megatons	600 Kilotons	+

* Source: Albert Wohlstetter, *Legends of the Strategic Arms Race*, USSI Report 75-1 (Washington, D.C.: United States Strategic Institute, 1975), p. 24. All other numbers taken from the body of this paper.

** Circular Error Probable, the number of nautical miles from target within which a warhead will land 50 per cent of the time.

obviously due in part to the inherent uncertainties in forecasting. Yet the record suggests certain patterns for mistaken estimates—some common sources of error and some lessons to be learned.

Sources of Error in Strategic Forecasting

As reconnaissance technology has improved

over the decades, U.S. intelligence has become more proficient in the science of collecting data. It has more "hard" information about the Soviet military-industrial establishment—missile deployments, production facilities, etc.—and, therefore, a firmer platform from which to make projections.

Yet, few of the mistakes noted in this retrospective have been due to errors in intelligence

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gathering; most are attributable to mishaps in the far more uncertain art of intelligence analysis. Here is where judgment comes into play—and it seems that, in several instances, misjudgment distorted the view of the future.

There are several principal sources of misjudgment.

Preconceived Notions. It is human to look at the world with preconceived notions—prejudices, excessive attention to some things, insufficient attention to others. These preconceptions shape what we look for and what we believe we see.

Occasionally, these preconceptions limit our vision. President Truman, General Groves and certainly others believed that it would take many years for the Soviets to build an A-bomb because they had a preconceived image of the Russians as technological primitives. With respect to error in forecasts of Soviet ABM, U.S. intelligence fell victim to a preconceived notion of what might be called "extended rationality." The members of the community knew that the Soviets traditionally emphasized defenses in their military program—it followed logically that Moscow would strive for a nationwide ABM. They recognized that the Tallinn site, with its SA-5 missile, was worthless for anti-bomber defenses—therefore, they concluded, assuming Soviet military planners to be flawlessly logical, that it *must* be an ABM system.

Mirror-Imaging. In the absence of obvious facts to the contrary, U.S. intelligence often strays into the assumption that the Soviets conceive of military problems in roughly the same way that American analysts do. This, too, is a natural and understandable human trait. It, too, can mislead.

U.S. intelligence underestimated the number of Soviet ICBMs, for example, because American analysts assumed that the Soviets, like the Americans, would stress quality rather than quantity in the further development of their strategic nuclear forces—specifically, opting for MIRVs on their missiles instead of building more missiles. It was also assumed that, like the United States, the USSR would replace old, obsolescent missiles with new ones. Instead, the Soviets chose a quantitative build-up of their missile forces and did not retire older ICBMs until much later.

Misjudgment of Soviet Strategic Priorities. The "bomber gap" and the "missile gap" were not the unqualified intelligence fiascos that they have been painted to be. The Soviets did produce and deploy hundreds of bombers in the late 1950s and hundreds of missiles in the early 1960s. The mistake was in assuming that Moscow would emphasize long-range strategic weapons aimed at the United States. In fact, Soviet strategists decided that areas along the periphery of the USSR—most notably in Western Europe--were the locus of the greatest

threat to the Soviet Union, and they accordingly concentrated on the development and production of medium- and intermediate-range weapons. The Eurasian peripheries, after all, represented the historical arena of threats to Russia—and until the 1960s these were the regions where most of the U.S. strategic strike forces were deployed.

In the case of Soviet ICBM forces in the 1960s, U.S. analysts did not underestimate the magnitude of the Soviet defense effort as much as they misjudged Soviet priorities. They believed that the Soviets would go for small numbers of heavy missiles, put more resources into quality than quantity, and emphasize defensive missiles. Thus, the intelligence community projected a large number of SS-9s, low numbers of ICBMs, early deployment of MIRVs and thousands of ABMs. Instead, the Soviets developed only a few hundred SS-9s and about 1,000 smaller SS-11s, took several years longer to field MIRVs, and halted their ABM program after only 64 were deployed.

Political and Bureaucratic Pressure. Intelligence is not practiced in a political vacuum. Direct political interference in National Intelligence Estimates is rare: the reported Baroody case, alluded to earlier, is the only one on record. Nevertheless, intelligence estimates are often highly responsive to the political needs of the client and to the politics of the moment, even when the heavy hand of politics is not visibly applied. The Air Force's need to justify its MIRV program was one factor in its projection that the Soviets would build 10,000 ABM interceptors. Likewise, the Nixon Administration's desire to deploy the Safeguard ABM system was one reason for its initial early estimate of the Soviet MIRV program.

The CIA's underestimation of Soviet ICBM deployments coincided with Defense Secretary Robert McNamara's public testimony that the Soviets would not try to match the U.S. force in number. This was his rationale for resisting pressures to expand U.S. nuclear forces—the level of which, having been arbitrarily set at 1,000 ICBMs, was difficult to justify convincingly as opposed to some equally arbitrary higher (or, for that matter, lower) level. McNamara did not have to signal CIA analysts directly in order to have his logic reflected in their estimates; they read the newspapers as carefully as the rest of the Washington bureaucracy.

Spurious Learning. Bureaucracy has been defined as an organization that cannot learn from its own mistakes.¹¹ The intelligence community's record in strategic forecasting bears this out. When the community reacts to previous errors, the lessons it "learns" are often spurious; the community overcompensates for its errors instead of revising the methods that produced them. Thus, overestimates tend to be

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followed by underestimates, and vice versa.

The underestimates of ICBM deployments in the 1960s were, in part, in response to the overestimates of the late 1950s. CIA officials were determined not to repeat the mistakes of the "missile gap." Similarly, the intelligence community shifted back and forth in its estimate of when the Soviets would deploy MIRVs. First they overestimated (in 1965, the expectation was 1970), then they underestimated (in 1968, the projection was 1978), then overestimated again (in 1969, the projection was 1971). The actual Soviet MIRV deployment came in 1975.

Failure to Use Soviet Sources. The charge that has been leveled against the CIA is that its estimators ignore clear statements of Soviet intentions and capabilities that are often to be found in the open Soviet literature.³⁴ This is a difficult issue to deal with. On some occasions, a heeding of Soviet statements would have made for more accurate intelligence. For example, in a public speech in July 1965, Brezhnev contended that the United States was underestimating the scope of Soviet ICBM programs—which turned out to be true. Similarly, if the CIA had given credence to the statement by the Soviet official that the new Soviet ICBMs had demonstrated a .27 nautical-mile CEP by 1974, the Agency would not have underestimated the rate of improvement in the accuracy of Soviet SS-18 and SS-19 missiles.

Still, some of the Soviet statements are clearly false: an example is Khrushchev's boast that the Soviets had a missile that could hit a fly in outer space. Such statements obviously must be tested against other available intelligence evidence. If the evidence does not match, however, should one then trust one's own estimates or the statements of a foreign government that has not been noted for its addiction to the truth?

Nobody has proposed a consistent set of rules for determining which Soviet statements are true and which are false. Some analysts follow the rule that any Soviet statement making the USSR appear hostile toward the United States is an accurate representation of Soviet intentions, while any less hostile statement represents a planned deception. This rule is obviously unsatisfactory for intelligence analysis.

Perhaps the CIA has been reluctant to grapple with the complexities of working with Soviet documents. There is no guarantee, in

any event, that correction of this deficiency will make intelligence analysis any more accurate or unbiased.

Implications of the Demise of SALT. Looking at the intelligence community from the outside, the public tends to visualize a machine spewing out facts. Rarely does the public realize that the intelligence community is composed of humans in a bureaucracy that is subject to the same pressures and pitfalls as any other.

The problems faced in making accurate forecasts are, of course, grounded first of all in the fact that the Soviet Union is a closed society and does not supply the world's libraries with volumes of public testimony from its generals about military plans. Given the limits on factual information that an intelligence system can draw from any closed society, the intelligence community must rely heavily on its analytical capabilities. This opens the product of the intelligence community wide to a host of human foibles—the preconceived notions, misjudgments, spurious "learning" and other shortcomings that have been discussed above. In fact, given the limited data base upon which the intelligence community must build its projections, it would not have been unreasonable to expect far more errors than have actually been committed.

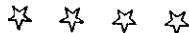
With increasingly more comprehensive SALT agreements, the intelligence community was finding its tasks made easier. The SALT agreements set concrete numerical ceilings for many categories of measurement of military power. The intelligence community did not have to rely on a murky crystal ball in examining every realm of Soviet activity. The SALT agreements narrowed the analysts' task: in those areas covered by SALT, they needed only to focus their capabilities and efforts on ascertaining whether the Soviets were adhering to their treaty pledges. Resources heretofore devoted to predicting future missile numbers could be devoted to other areas not covered by SALT.

With the death of SALT II, analysts must dust off once again the murky crystal ball. Estimates of future Soviet activity are likely to be wider off the mark than they would be under a SALT II Treaty, simply because the reference points provided by the Treaty have been removed. The human element is maximized, and with it the likelihood of human foibles increases.

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NOTES

1. Michael Getler and Robert G. Kaiser, "Intelligence Estimate Said to Show Need for SALT," *Washington Post*, January 31, 1980.
2. This cuts both ways. Acceptance of the estimate may lead to greater spending in some strategic arms, but it may also lead to rejection of the land-based multiple-shelter basing scheme for the MX missile on grounds that too many shelters would have to be constructed to "absorb" so many Soviet ICBM warheads.
3. The quotes are from Lt. Gen. Daniel O. Graham, former Director of the Defense Intelligence Agency, "Intelligence, Realities and Myth," *Wall Street Journal*, March 11, 1977, p. 16; George B. Kistiakowsky, "False Alarm: The Story Behind SALT II," *New York Review of Books*, March 22, 1979. Kistiakowsky was a member of the President's Science Advisory Committee, 1957-1963.
4. See Lawrence Freedman, *U.S. Intelligence and the Soviet Strategic Threat* (Boulder, Colo.: Westview Press, 1977), p. 64; Richard G. Hewlett and Oscar E. Anderson, *History of the AEC, Vol. I: The New World, 1939-1946* (University Park, Penn.: Pennsylvania State University Press, 1962), pp. 358-360; Herbert York, *The Advisors: Oppenheimer, Teller and the Superbomb* (San Francisco: W.H. Freeman & Co., 1976), pp. 34-36.
5. Cited in Freedman, *op. cit.*, p. 64, and information from D.A. Rosenberg, a researcher studying the U.S. decision to develop the H-bomb.
6. U.S. Senate Select Committee to Study Governmental Operations with Respect to Intelligence Activities (the "Church Committee"), *Final Report, Book IV, Supplementary Detailed Staff Reports on Foreign and Military Intelligence* (Washington, D.C.: Government Printing Office, 1976), p. 56; Freedman, *op. cit.*, p. 67; *The Military Balance, 1975-1976* (London: International Institute for Strategic Studies, 1975), p. 73.
7. For the text of NSC-68, see U.S. Department of State, *Foreign Relations of the United States, 1950, Vol. I: National Security* (Washington, D.C.: Government Printing Office, 1978), pp. 235-292; for the 200-bomb estimate, see p. 251. For a thorough analysis of NSC-68, see Paul Y. Hammond, "NSC-68: Prologue to Rearmament," in Warner R. Schilling, Paul Y. Hammond and Glenn H. Snyder, *Strategy, Politics and Defense Budgets* (New York: Columbia University Press, 1962).
8. Freedman, *op. cit.*, pp. 65-66.
9. Ibid.
10. Ibid.
11. Ibid.
12. Even now, the Soviets emphasize medium-range far more than long-range bombers. They have 156 Bear and Bison long-range bombers, compared with 613 Badger medium-range bombers and 80 Backfires that have limited intercontinental capability but are deployed and exercised mainly for "theater" missions. See *The Military Balance, 1979-1980* (London: International Institute for Strategic Studies, 1979), p. 89.
13. This section is based on material in Edgar M. Bottome, *The Missile Gap: A Study of the Formulation of Military and Political Policy* (Cranbury, N.J.: Fairleigh Dickinson University Press, 1971); Freedman *op. cit.*; Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (Chicago: University of Chicago Press, 1966).
14. Material in this section is based on Freedman, *op. cit.*, Chapter 5; Edward R. Jayne II, *The ABM Debate: Strategic Defense and National Security* (Cambridge, Mass.: MIT Center for International Studies, 1969); Ronald Tammen, *MIRV and the Arms Race* (New York: Praeger, 1973).
15. Paul H. Nitze, commenting on articles by Albert Wohlstetter, *Foreign Policy*, Fall 1974, p. 82.
16. See Freedman, *op. cit.*, p. 116.
17. *Ibid.*, pp. 137 ff.; Senate Foreign Relations Committee, Hearings, *Intelligence and the ABM* (1969), p. 24; John Newhouse, *Cold Dawn. The Story of SALT* (New York: Holt, Rinehart & Winston, 1973), p. 161.
18. Melvin R. Laird, Department of Defense, *Fiscal Year 1971 Defense Program and Budget* (February 20, 1970), p. 39.
19. Albert Wohlstetter, "Is There a Strategic Arms Race?" and "Rivals But No Race," *Foreign Policy*, Summer and Fall 1974. These articles were reprinted in *Strategic Review*, Fall 1974 and Winter 1975, and then published together as *Legends of the Strategic Arms Race*, USSI Report 75-1 (Washington, D.C.: United States Strategic Institute, 1975). The Wohlstetter articles sparked a debate involving articles and replies by Paul Nitze, Joseph Alsop, Morton Halperin, Jeremy Stone, Michael Nacht and Johan Holst, in *Foreign Policy*, Fall 1974 and Summer 1975.
20. U.S. Congress, Joint Economic Committee, Hearings, *Allocations of Resources in the Soviet Union and China—1975*, Part I, pp. 97-98.
21. Robert S. McNamara, Department of Defense, *Posture Statement for FY 1964* (February 1963), p. 22.
22. Freedman, *op. cit.*, p. 146.
23. Robert S. McNamara, Department of Defense, *Posture Statement for FY 1967* (1966).
24. An index of vulnerability has been calculated as Y^2 divided by CEP^2 , where Y = weapon yield in megatons, and CEP = Circular Error Probable, or the distance from the target within which a warhead will land 50 per cent of the time. The gist is that increasing yield or accuracy will boost a warhead's "kill probability" against a target, but that doubling accuracy will have the same effect as boosting yield by eight times. See Kosta Tsipis, *Offensive Missiles* (Stockholm: Stockholm International Peace Research Institute, 1974).
25. Newhouse, *op. cit.*, p. 129.
26. Freedman, *op. cit.*, p. 141.
27. See U.S. Senate Select Committee to Study Governmental Operations with Respect to Intelligence Activities ("Church Committee"), *Final Report, Book I, Foreign and Military Intelligence* (Washington, D.C.: Government Printing Office, 1976), pp. 77-79, and Laurence Stern, "Agency Forced to Alter Own Data," *Washington Post*, April 27, 1976.
28. Michael Getler, "Russian Missile Faulted," *Washington Post*, June 17, 1971.
29. Freedman, *op. cit.*, p. 173; U.S. Senate Foreign Relations Committee, Hearings, *Briefings on Counter-force Attacks* (September 1974).
30. According to data released by Paul Nitze, the newest version of the SS-18 (the 10-warhead Mod-4 variant) has a CEP of .17 nautical miles, as does the latest SS-19 with 8x warheads. See Nitze's testimony, Senate Foreign Relations Committee, Hearings, *The SALT II Treaty* (July 1979), p. 459.
31. Walter Pincus, "U.S. Downgrades Soviet ICBM Yield," *Washington Post*, May 31, 1979.
32. Albert Wohlstetter, *Legends of the Strategic Arms Race*, *op. cit.*, p. 14.
33. Michael Crozier, *The Bureaucratic Phenomenon* (Chicago: University of Chicago Press, 1964).
34. See for example the views of Senator Malcolm Wallop, in U.S. Senate Select Committee on Intelligence, *The National Intelligence Estimates A-B Team Episode Concerning Soviet Strategic Capability and Objectives* (Washington, D.C.: Government Printing Office, February 1978), p. 13.



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DEBATE OVER U.S. STRATEGIC FORECASTS: A POOR RECORD

WILLIAM T. LEE



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IN BRIEF

Congressman Aspin's assessment, while heralding a welcome Congressional attention to the problems of U.S. strategic forecasting of Soviet weapons developments, does not portray accurately the U.S. intelligence community's past performance in this crucially important arena. His scoring of the eight cases of forecasting selected not only is too generous to the CIA and other U.S. intelligence agencies, but it also neglects the relative weight of the mistakes committed—particularly in the failure to forecast the formidable build-up of Soviet strategic capabilities in the 1970s. The record of intelligence estimates becomes even more grievous when looked at in the larger compass of the CIA's responsibilities, notably its estimates of Soviet defense expenditures. A ratified SALT II Treaty could not ease the problem; the solution, rather, lies in badly needed improvements in the intelligence interpretation of the ample evidence available.

Congressman Les Aspin has offered an assessment of U.S. intelligence forecasts of Soviet weapons systems development and deployment that says, in effect: We have won some, lost some and tied some. Moreover, he implies that this is about the best one can expect from intelligence forecasting of Soviet weapons technology and deployments.

Congressman Aspin's assessment is welcome on three counts. First, it needs pointing out, as he does, that the U.S. intelligence services have a mixed record in forecasting Soviet weapons development: there have been some overesti-

mates as well as many underestimates. Second, public recognition of some of the intelligence underestimates by a member of the Congressional Select Committees on Intelligence is long overdue. Third, at a time when the Congress and the Executive Branch are negotiating a charter to govern the activities of covert intelligence collection and action, our attention needs to be focused also on the neglected question of how to accomplish improvements in the intelligence analysis and projection of Soviet forces that threaten our survival. We are about a decade late in realizing that the repeated and

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systematic misestimates of future Soviet forces by the analytical components of the intelligence community have had far more serious negative consequences for U.S. national security than whatever excesses the covert intelligence components may have engaged in. Moreover, unlike a recent assessment by Messrs. Robert Ellsworth and Kenneth Adelman,¹ Congressman Aspin does not blame the covert operators of the CIA for the failures of the Agency's intelligence analysts. It is hoped, therefore, that Congressman Aspin's assessment represents the beginning of a serious bipartisan effort by the Congress, in cooperation with the Executive Branch, to improve the quality of U.S. intelligence analysis, particularly long-range forecasting.

On the negative side, Mr. Aspin's assessment is less than comprehensive, representing a very small sample of intelligence community forecasts of Soviet military programs. A number of instances where the CIA has unambiguously or inferentially underestimated Soviet programs are conspicuous by their absence from Congressman Aspin's review. He also fails to note that the most grievous underestimates of Soviet programs have occurred since the revolution in intelligence collection capabilities that he dates to August 1960. Finally, certain "facts" cited by Congressman Aspin require further (probably official U.S. Government) documentation before they can be accepted as such.

Quantitative Measures of Performance

Congressman Aspin evaluates U.S. intelligence estimates of Soviet weaponry on eight counts—four forecasts of Soviet strategic missile and aircraft deployment, and four forecasts of critical nuclear and missile technologies. Table 1 attempts to summarize Congressman Aspin's assessment of the U.S. intelligence community's performance. The "box-score" that is reflected seems consistent with one of Congressman Aspin's summary statements: "... the intelligence community overestimated Soviet capabilities on three occasions, underestimated them once, and both over- and underestimated in two cases. The community was almost exactly on target once and divided between accuracy and underestimation once."

Two general observations are in order. First, this is far too small a list of cases on which to base an evaluation of the effectiveness of the CIA and other members of the U.S. intelligence community in forecasting Soviet forces and technologies five or ten years into the future. Second, one must take issue with Congressman Aspin's account, as presented, of the record of initial intelligence forecasts made during the 1960s of Soviet MIRV deployment, and of the

Table 1

Congressman Aspin's Box-Score of U.S. Intelligence Forecasts of Soviet Military Technologies and Weapons Systems Deployments

Technology/ System	Under- estimate	Over- estimate	Near the Mark
1st A-Bomb Test	X		
1st H-Bomb Test			X
"Bomber Gap"		X	
"Missile Gap"		X	
ABM Deployments		X	
ICBM Deployments	X		
MIRV Deployments	X		
ICBM Accuracy and Yields (since 1969)	X	X	

Definitions:

Underestimate: The Soviets achieved a capability earlier, or deployed a larger force, than the U.S. intelligence community forecast.

Overestimate: The opposite of underestimate.

Near the Mark: A relatively small discrepancy between forecasts and observed events.

accuracy and yield of Soviet MIRVed ICBMs when these systems were deployed in the mid-1970s.

In each decade of the 1960s and the 1970s the Soviets procured some 100 major weapons systems—offensive and defensive, of strategic reach and shorter ranges. A number of technologies, some standard and others that were more or less advanced, were incorporated into each of these weapons systems. In each of the two decades, about half of the weapons systems procured had been developed in the previous decade.

This pattern is likely to continue through the 1980s.² Consequently, four Soviet weapons development programs and four technologies represent a rather small sample—even if one wants to restrict the evaluation of the intelligence community's prowess to "strategic" weapons systems and technologies. Such a restriction of the field of inquiry is much too narrow. In terms of overall military capabilities, "tactical" systems are as important as, if less glamorous than, "strategic" systems. Both are equal parts of the intelligence community's charter. Both weigh on the U.S.-USSR military balance and, most importantly, in the perceptions of that balance by the U.S., the USSR, their respective allies, and the "third world." Nor does Mr. Aspin mention the CIA's abysmal record in underestimating Soviet defense expenditures and long-term trends in national economic priorities.

Before turning to the question of how the intelligence community's record would look if a

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larger sample of forecasts of Soviet weapons programs were used, let us examine Congressman Aspin's eight cases in a little more detail. Congressman Aspin's count of intelligence forecasting successes and failures is sensitive to his definition of terms. He recognizes that both the "bomber gap" and the "missile gap" embrace two distinct families of weapons systems in each case: medium and heavy bombers, and IR/MRBMs and ICBMs, respectively. Both appear as overestimates in Table 1.

Yet, if we split the "bomber gap" and the "missile gap" into their two components, heavy and medium bombers and ICBMs and IR/MRBMs respectively, then the count for underestimates increases from five to seven because U.S. intelligence services *underestimated* Soviet medium bomber and IR/MRBM programs by roughly the same degree that they *overestimated* heavy bombers and first generation ICBMs. Mr. Aspin excuses U.S. intelligence services on the grounds that they did not grasp either Soviet strategic priorities or the Soviet perception that the immediate threat to the USSR emanated from NATO and U.S. overseas bases rather than from the aircraft and missiles based in the continental United States.³

This is true, indeed. U.S. intelligence has conspicuously failed to understand Soviet strategic concepts, perceptions and priorities. Soviet IR/MRBMs and medium bombers, which are considered by the Soviets just as "strategic" as ICBMs and heavy bombers, continue to be referred to in the United States as "gray area" and "peripheral" systems. For more than a decade the United States has put all of its strategic systems into the hamper of SALT negotiations while allowing the Soviets to withhold about one-third of their strategic systems from the bargaining process. The persistent use of the terms "peripheral" and "gray area" to describe Soviet strategic missiles and bombers, the exclusion of about one-third of Soviet strategic forces from the strategic arms control agreements negotiated with the Soviets over the past decade, all indicate that both the CIA and most of the intelligence analysts in other agencies of the U.S. Government did not learn much from the so-called "bomber" and "missile" gaps.

If the CIA had absorbed the lesson of the unexpectedly large Soviet medium bomber and IR/MRBM deployments in the late 1950s and early 1960s, the Agency might not have underestimated Soviet ICBM deployments from the mid-1960s onward. Soviet IR/MRBM force levels—some 700 launchers—were designed to meet the requirements of Soviet nuclear targeting strategy. Widely publicized after 1962, Soviet targeting strategy required not only the 1,600-odd ICBMs the Soviets eventually deployed, but also MIRV technology because the Soviets could not afford to deploy sufficient

numbers of ICBMs with one warhead each in order to satisfy their required capabilities against either hard or soft targets. As has been pointed out previously, taking into account Soviet requirements is a prerequisite for effective long-term forecasting of Soviet strategic force deployments.⁴

Congressman Aspin's history of the intelligence community's estimates of Soviet MIRV programs is questionable in part and less than reassuring *in toto*. According to Mr. Aspin, U.S. intelligence services made the following forecasts of Soviet MIRV technology:

- The 1965 National Intelligence Estimate (NIE) predicted MIRVs would be operational on Soviet strategic missiles in 1969-1970.
- Three years later, the 1968 NIE moved the operational date for Soviet MIRVs to 1978.
- The 1969 NIE (prepared in 1968) estimated Soviet MIRVs could be operational as early as 1971.⁵
- In September 1969 the CIA's Board of National Estimates concluded that "the Soviets could not, and would not try to, achieve a first-strike capability against Minuteman during the 1970s."

Congressman Aspin provides no documentation for this chronology. Even if one accepted it as an accurate record, the CIA's vacillation on the MIRV issue is staggering. Consider once again the alleged sequence as outlined above. In 1965, Soviet MIRVs were predicted for 1969 or 1970. In 1968, they were not projected a threat until 1978. In the 1969 NIE suddenly the conclusion was reached that the Soviets would have MIRVs in 1971. If this is a valid history of the sequence of CIA and NIE forecasts of Soviet MIRVs, then one has to wonder about the sobriety of the estimators.

Yet some who were involved in the 1965 NIE do not remember it this way. According to their account, the Air Force argued that the Soviets would develop MIRVs by the end of the 1960s. As usual, the Air Force arguments were derided by CIA analysts. As usual, the Air Force deposited its footnote to the NIE, this time contending an early development of Soviet MIRV capabilities. According to a senior Air Force analyst who participated in the 1965 NIE on Soviet strategic offensive weapons systems, the NIE did not forecast Soviet MIRVs for 1969-1970.

In retrospect, the Soviet MIRV program probably was approved in about 1965. Feasibility tests evidently were carried out in two 1964 space shots.⁶ Hardware for the first flight test

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—in August 1968—of the triplet version of the SS-9 ICBM probably was being fabricated by the end of 1965 at the latest.⁷ The development of the SS-16, SS-17 SS-18 and SS-19 ICBMs must have been approved no later than 1966, probably by the end of 1965 as part of the 1966–1970 Five Year Plan, in order to allow for the flight test programs that began in 1972.

The FY 1971 Department of Defense Posture Statement confirms that by 1969 the NIEs expected Soviet hard-target MIRVs as early as mid-1971—a date which turned out to be about three years premature.⁸ In this case, however, the intelligence community evidently had a good excuse, which Congressman Aspin does not mention: by early 1970 the community had detected construction of new types of silos in Soviet ICBM complexes.⁹ Presumably this was taken as an indicator that flight tests of new model ICBMs were imminent. This, in turn, could have been interpreted to mean that one or more new-model ICBMs would become operational by the end of 1971. In any case, the 1969 strategic force NIE evidently did forecast a Soviet MIRV capability by as early as mid-1971. The new silo construction detected by early 1970 quite understandably could have induced such a prediction—although, given the complexity of MIRV technology, mid-1972 would have been a more prudent date.

Yet, Congressman Aspin reports that in September 1969 the Board of National Estimates wrote that the Soviets "could not, and would not try to" achieve the capability to attack Minuteman silos (effectively) in the decade of the 1970s. If true, this represents an extraordinary confusion. Within a period of less than twelve months in 1968–1969 the CIA's Board of National Estimates took this position while the NIE predicted, without making a choice, either Soviet counter-silo MIRVs by mid-1971, or counter-silo multiple RVs in 1974.¹⁰

If true, all this also constitutes a most damning indictment of the CIA. An overestimate of three years for initial Soviet MIRV capabilities in the earlier 1969 NIE was not a grievous error: it was at least on the right track. But to argue in September 1969 that the Soviets could not and would not pose a threat to U.S. ICBMs in the 1970s was a gross underestimate that (1) flew in the face of all evidence *then* available, and (2) was fraught with serious policy implications that continue to haunt us today. If it was advanced, as Congressman Aspin reports, the Board of National Estimates' argument of September 1969 exemplifies the disregard for evidence and the proclivity for "mirror imaging" that most concerns those critics who have concluded that the CIA could and should have done better—and who believe that the strategic predicament in which the U.S. finds itself today is due in no small part to such badly

mistaken estimates, in which the CIA played the dominant bureaucratic and intellectual roles within the intelligence community.

If in September 1969 the CIA Board of National Estimates so grossly misperceived the thrust of Soviet strategic missile programs into the 1970s, it was not from lack of evidence. In the context of that evidence, the arguments at that time over the accuracy of the "triplet" version of the SS-9 have little relevance. The CIA probably was right in its estimate of the accuracy of that missile, as tested in 1969. Department of Defense officials probably also were correct in believing that the accuracy could be improved sufficiently with a few more years of testing to make the SS-9 a silo-killer. The evidence available in the public domain marks this argument as one of those tempests in a teapot which so often has muddied long-range estimates in the intelligence community. Soviet strategic objectives and the long-term trends on this issue were quite clear by 1969.

As has been noted, Soviet targeting strategy rated the enemy's nuclear delivery systems as the first priority. The Soviet IR/MRBM force of some 700 launchers was sized for counter-force operations against nuclear delivery systems and other military targets in NATO territory and elsewhere along the Eurasian periphery. One or two hundred IR/MRBMs would have been more than adequate if the Soviets had been interested only in targeting cities in a minimum deterrent strategy. The SS-9 had been predicted as a missile designed to attack hardened Minuteman launch-control centers. Both the design and the deployments validated that prediction. The triplet version of the SS-9 made sense only as an attempt to acquire a system which could attack individual Minuteman silos even if the necessary accuracy was not achieved. If the CIA's Board of National Estimates failed to understand Soviet targeting priorities by 1969, it was not because of lack of evidence.

Congressman Aspin's observation that the Soviet counterforce threat that emerged in the 1970s had not been taken seriously in the 1960s, at least not in the NIEs, is supported by Secretary Robert McNamara's 1967 Posture Statement.¹¹ Because the NIEs were not forecasting a threat to the U.S. Minuteman ICBMs in the 1970s, Defense Department analysts invented the "greater than expected threat" in order to help justify U.S. MIRV programs. This threat projection deliberately went far beyond that of the NIEs. Had the DoD planners not exercised such prudence, the United States probably would have faced a "MIRV gap" in the early 1970s.

Yet, the Defense Department planners went one step further: in an effort to discredit various options then (in the mid- to late-1960s)

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being considered for deploying American anti-ballistic missile defenses (ABM), they invented the "greater than the greater than expected threat." They contrived this threat by feeding into the computer the assumptions needed to arrive at the predetermined answer—namely, that U.S. ABM deployments not only would fail to reduce U.S. fatalities in the event of a Soviet attack (which, despite all the evidence to the contrary, was assumed to be designed to maximize civilian fatalities) but that such deployments would even result in increases in the fatalities. This contrived Soviet threat served as the basis for a table in Mr. McNamara's 1967 Posture Statement, which painted the horrendous picture of 70 to 120 million casualties on both sides as the inevitable consequence of a U.S. ABM deployment.¹² For reasons best known to the Agency, but having no relation to the available evidence, the CIA in 1967 gave its blessings to the "greater than the greater than expected threat."

In sum, the scoreboard of U.S. intelligence forecasts of Soviet MIRV technology reads quite differently from that presented by Congressman Aspin. The 1965 NIE probably did not overestimate the time that it would take the Soviets to achieve a MIRV technology. The early 1969 NIE probably did overestimate it, by about three years, but for understandable reasons. Nevertheless, both that NIE and later versions failed to forecast the full scope of Soviet counterforce capabilities that emerged in the late 1970s. Indeed, if Congressman Aspin is correct in his description of the late 1969 NIE and its optimistic predictions of both Soviet strategic intentions and ICBM capabilities through the 1970s, then this must be rated as one of the most dismal chapters in the history of U.S. intelligence. It bears out the obvious fact that errors in forecasting cannot be rated on the same scale: some are more serious than others. Consequently, instead of charging the U.S. intelligence services simply with one overestimate and one underestimate, it seems more realistic to score one "near the mark" for U.S. forecasts of MIRV technology and one grievous underestimate with respect to the potency of Soviet ICBM capabilities in the 1970s.

Finally, Congressman Aspin counts one underestimate for the U.S. intelligence community's judgment of the accuracy of the MIRVed ICBMs introduced by the Soviets in the 1970s and one overestimate with respect to the yields of those weapons. These cases concern *current* intelligence estimates of Soviet weapons characteristics, not *forecasts* of future developments. Over the years the estimated CEPs (circular error probable) of Soviet MIRVed ICBMs have shrunken from about 1,800 feet to between 600 to 900 feet as the result of a vigorous Soviet development effort.¹³ It is worth noting that the Soviet Union probably was able

to improve the accuracy of its MIRVed ICBMs only because the United States sold to the Soviets the precision machine tools needed to produce the requisite small, very precise ball bearings for Soviet missile guidance systems.¹⁴

As regards U.S. estimates of Soviet MIRV warhead yields, Mr. Aspin probably is both more mistaken and more correct than he realizes. He refers to a lowering of U.S. intelligence estimates of the yield on the SS-18 MIRVs from 1.5 to 0.6 MT. While it appears that there has been some scaling down in yield estimates, has it been of this magnitude? Caution is in order because the SS-18 is reported to have been tested with various MIRV payloads involving 8 and 10 reentry vehicles (RVs). Before Mr. Aspin can charge the intelligence analysts with in effect having originally overestimated the yield of the SS-18 RVs by a factor of two and one-half, he needs to demonstrate that the figures of 1.5 and 0.6 megatons refer to one and the same RV model. This he has not done.

On the other hand, it is probably true that intelligence community estimates of the yields of Soviet strategic missile RVs tend to be overstated *on the average*. When aiming at hard targets, the Soviets need all the yield they can pack into the RV. Consequently, the single RV version of the SS-9 probably did carry a warhead on the order of 20 MT, and the three-RV version probably delivered warheads with yields of approximately 5 MT. Yet, megaton and multimegaton warheads make no sense against soft targets—and it is important to keep in mind that Soviet targeting strategy has clearly deemphasized strikes against the general U.S. population and heavy industry.¹⁵ As regards such "soft" (urban-industrial) targets, the Soviet literature specifies the use of minimum yields.¹⁶ Consequently when aimed at soft targets, even the older and relatively inaccurate Soviet ICBMs and IR/MRBMs seldom required yields of even one megaton: depending on the degree of damage required, yields in the range from 0.05 to 0.5 MT were adequate. Given the accuracy of current Soviet MIRVs, 0.15 MT is about the most yield that is required for soft targets, and the lower limit drops to about 0.025 MT.¹⁷

Apparently the intelligence community has been estimating Soviet strategic missile yields at the maximum levels considered to be technically feasible. This is probably a reasonable approach for estimating the explosive power of missiles designed to attack hard targets, such as the SS-9, SS-18, and SS-19. But it probably overestimates, by as much as an order of magnitude or more, the yields the Soviets are likely to employ against soft targets.

Before turning to a more comprehensive survey of intelligence performance, it may be appropriate to deal with one of the alibis offered

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for the CIA's quantitative underestimates of Soviet ICBM deployments: namely, that the assumption was made in each succeeding NIE that the Soviets would retire some 220 older SS-7 and SS-8 ICBMs. If these are added to the annual estimates, Congressman Aspin argues, the NIEs were not too far off base.

No documentation is offered for this alibi. Moreover, the notion that *every* succeeding NIE expected *all* 220 of these ICBMs to be retired seems too neat an excuse. What was the basis for such an expectation—if indeed it was entertained and so consistently? Throughout the mid-1960s and until the late 1970s the Soviets did not have nearly enough ICBM warheads to cover the whole array of both hard and soft targets in the United States. Their reserve also was inadequate.¹⁴ Until several years ago, the Soviets needed every one of those old ICBMs, and there should have been no question that the missiles could be kept in service that long. The United States still has 54 Titan II ICBMs in its inventory and evidently intends to keep them until the mid-1980s at least.

Indeed, the Soviets had no difficulty in maintaining their SS-4 and SS-5 missiles—many of which antedated the SS-7s and SS-8s—in service during the 1960s and 1970s. True, these missiles were soft and vulnerable, but what did that matter in the context of a preemptive strike strategy—to say nothing of the launch-on-warning concept which the Soviets adopted in the mid-1960s?¹⁵ The whole excuse is threadbare. And if the CIA really clung so stubbornly to the expectation that the Soviets would retire those SS-7s and SS-8s, then this represents no excuse at all, but simply another mistaken estimate to be added to all the others.

A Wider Appraisal of Strategic Estimates

- Up to this point we have addressed only Congressman Aspin's "box-score" of U.S. intelligence forecasts of Soviet weapon technologies and systems deployments. Closer examination already has changed the roughly "even" score adduced by Mr. Aspin to one that gives the edge to underestimates by the intelligence community. But to gain a more accurate appraisal, we need to consider the CIA's record on several other strategic issues and programs which Mr. Aspin omitted from his analysis.

Thus, while he concedes that the CIA underestimated Soviet ICBM deployments from the mid-1960s through the early 1970s, Congressman Aspin overlooks parallel intelligence underestimates of Soviet SLBM programs. Such underestimates emerge even from Department of Defense Posture Statements, and additional documentation of what has long been an open secret has been provided by a former CIA analyst, Ray Robinson.²⁰ In the mid-1960s the CIA officers adamantly rejected evidence that

the Soviets had a large SLBM program underway—one that eventually resulted in the deployment of 34 Y-Class SSBNs carrying 544 SS-N-6 SLBMs. At one point Robinson was asked to resign for the sin of sticking by the evidence.²¹ Thus, one more major underestimate must be added to the record.

Later, in the mid-1970s, MIRVs for Soviet SLBMs evidently were not forecast. In his FY 1977 Posture Statement, Secretary Rumsfeld noted: "There is no evidence that any missile other than the 4,200 nm single-RV SS-N-8 will be carried in the near term" by the 16-tube version of the Delta Class SSBN.²² Yet, in his FY 1978 Posture Statement, Secretary Rumsfeld reported the testing of the SS-N-18 MIRV,²³ which probably became operational in late 1977 or early 1978. The Soviets admitted to having 144 of the MIRVed missiles deployed by June 1979, when they signed the SALT II Treaty.²⁴ So another underestimate must go up on the scoreboard.

At this point, the score reads three overestimates (heavy bombers, and first-generation ICBMs and ABMs in the 1960s) and six underestimates (medium bombers, IR/MRBMs, third-generation ICBMs, SLBMs, the counter-force threat from Soviet ICBM MIRVs and MIRVs for SLBMs). The gist of this record is that not a single strategic missile or aircraft *deployment program* appears to have been estimated correctly in the course of the past twenty-five years. The batting average reads no hits in nine times up—not exactly an all-star performance.

CIA estimates of Soviet defense expenditures represent another area of significant errors—one to which, curiously, Congressman Aspin does not allude. Perhaps the reasoning is that the CIA's defense expenditure estimates were not forecasts. If this is the explanation, it is not convincing for two reasons. First, it is inconsistent: the estimates of Soviet MIRV accuracy and yields that Congressman Aspin included in his assessment were "current estimates" of existing Soviet systems, not forecasts. Second, the CIA does make long-term forecasts of Soviet defense expenditures.

From 1960 to 1975 the CIA estimated Soviet defense spending, excluding RDT&E and space, at essentially the same levels as the published Soviet "defense" budgets—notwithstanding the evidence that since at least 1951 the latter figure had not included weapons procurement. Moreover, between 1960 and 1975 the CIA probably spent between \$0.5 and \$1.0 billion of taxpayers' dollars in producing an estimate of Soviet defense expenditures that was readily available on the shelves of the Library of Congress and many university libraries. Time and time again the CIA argued that its estimates were correct and denounced both critics and criticism of its methods and its estimates.

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In 1975 the CIA discovered that its estimates of Soviet defense spending in 1970 had been off by a factor of two—that the USSR had spent at least 50 billion rubles rather than the 24 billion rubles which the CIA had estimated previously. About the same time, the CIA discerned that its estimates for 1972 were off by somewhat more than a factor of two—amounting to some 58 billion rubles rather than the 27 billion rubles estimated. In 1976 the CIA published its revised estimates for Soviet defense expenditures in 1970. Within this revision of the total, the CIA tripled its previous estimates of Soviet military procurement in 1970: from between 5 and 6 billion rubles to about 17 billion rubles.

The new information, however, only told the CIA its aggregates had been too low by factors of two to three: it apparently did not tell the CIA how to correct its ponderous direct-costing methodology. This writer's pleas to the Agency to correct its methodology before publishing its revised estimates were rejected. In 1976 the CIA published its revised estimates which—to oversimplify only slightly—represented merely the prior estimates moved up the scale. Predictably, the revised CIA estimates showed only a slightly higher rate of growth in Soviet defense spending than its prior estimates. (Any Soviet defense posture falling between unilateral disarmament and full mobilization registers a 3 to 4 per cent per annum growth rate on the CIA's direct-costing model.) Soviet defense expenditures for the years prior to 1970 now were overestimated; after 1970 they were underestimated as before. By 1972 the revised CIA estimates were already about 20 per cent short of the 58 billion rubles which Brezhnev reportedly had indicated the Soviets spent for defense that year.²⁵ In 1980 the revised CIA estimates are once again off by a factor of two, or nearly so.

As before, the CIA estimates of Soviet defense expenditures are produced at considerable cost to the taxpayer, but can be replicated (in the aggregate) by almost any analyst with minimal effort. Total CIA personnel costs alone for producing these estimates probably are on the order of \$5 million annually. To replicate the revised CIA estimates, simply plot its prior estimates on a piece of graph paper. Move the prior 1970 estimate of 24 billion rubles up to the revised estimate (mid-point) of 45 billion rubles. Through this point draw a line parallel to the prior estimate. This line represents the announced Soviet defense budget. It is a very cost-effective method, indeed.

The score of the CIA's underestimates is now increased by two: one for the prior estimates of Soviet defense expenditures and one for the revised estimates. To be sure, the CIA has not yet admitted to the second underestimate, but the evidence for it is as good as or better than before. It may take another decade or two for

the CIA to discover its new error—by which time its estimates may be awry by a factor of three or four or more.

Before leaving the arena of strategic estimates, a few words may be in order on the subject of Soviet anti-submarine warfare (ASW) programs. With only one exception, every major surface combatant vessel that has been converted or constructed for the Soviet Navy since the early 1960s has been assigned ASW as a primary mission. (Soviet designators are "BPLK" and "PLK" for "large anti-submarine ship" and "anti-submarine cruiser.")²⁶ It remains to be seen whether the same will apply to the four new classes of Soviet surface combatants currently reported to be fitting out or under construction, but it is likely that most and perhaps all will be equipped and trained for the ASW mission.²⁷ Similarly all Soviet SSNs not equipped with cruise missiles that have been built since the late 1950s have been assigned the strategic ASW mission.

Did the CIA forecast such a Soviet emphasis on ASW even in the late 1960s and on into the 1970s? The answer is: apparently not. The annual Department of Defense Posture Statements have reported the construction of each class of new Soviet surface combatant and SSN as it appeared, but nothing can be inferred from the Statements that suggests intelligence forecasts of the vast ASW effort that has dominated Soviet ship construction. Little or no heed was paid to the characteristics of the Alpha-Class SSN as they were described by two Soviet admirals in the General Staff journal, *Military Thought*, in the late 1960s.²⁸ Series production of this SSN in the late 1970s evidently came as a surprise: even Secretary Harold Brown's FY 1981 Report indicates such production only by implication, noting that the Soviets are now producing 10 "general purpose" SSNs per year.²⁹ In fact, in the Soviet view all of these SSNs are "strategic" systems. Unless it can be demonstrated that the DoD Posture Statements somehow have ignored intelligence forecasts of the Soviet emphasis on strategic ASW over the past two decades, we must chalk this up as another CIA underestimate.

Forecasts of Soviet General Purpose Force Programs

This brings us to CIA forecasts of major weapons programs for Soviet Ground Forces, Frontal Aviation (FA) and Military Transport Aviation (VTA). Did the NIEs between 1968 and 1971 prepare U.S. planners for the contingency observed by Secretary Rumsfeld in the FY 1977 Annual Report that "This total force appears to be larger than would be required for even the most stalwart of defenses . . ."³⁰ —or for the similar observations made by

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Secretary Brown in the FY 1981 Annual Report?³¹

It seems clear from the various Posture Statements by the U.S. Secretaries of Defense that the NIEs written in the late 1960s and early 1970s forecast neither the increase in the number of weapon systems introduced into the Soviet Ground Forces, Frontal Aviation and Military Transport Aviation during the 1970s, nor the quantities in which they were produced, nor the advances in technological sophistication which went into the Soviet weaponry. Each new weapon system was reported as it appeared in the 1970s, but the whole thrust of the Soviet Union's effort in increasing and modernizing its general purpose forces evidently was not discerned by the CIA until after its implementation was well advanced. Even Secretary Rumsfeld's FY 1977 Report did not anticipate the new family of Soviet tactical surface-to-surface missiles which have been or are currently replacing the older models.³²

Learning From Mistakes

This brief survey is about as far as one can proceed within the bounds of what has been reported and what can be inferred from the material available in the public domain with respect to CIA and intelligence community forecasts of Soviet weapons deployments and technologies. Table 2 attempts to summarize the historical record of intelligence forecasts and estimates as best as can be deduced from the available evidence in the public domain. The record is flattering neither to the CIA nor to the intelligence community as a whole, nor can it be reassuring to the policymaker or to the taxpayer. The "best and brightest" should have done better.

Predicting the future always will be a difficult enterprise. It is an occupation in which, in order to be right, one has to have the courage to be wrong. Under the best of efforts and circumstances, no one can realistically aspire to a flawless record in forecasting. Nevertheless, the record ought to be substantially better than is suggested in Table 2.

One obstacle to improvement seems to be the reluctance, particularly in the CIA, to admit errors. Intelligence is a business in which one learns more from one's mistakes than from one's successes. But in order to learn from past errors one must first recognize them and then analyze them. Some time ago a former Deputy Director of the CIA's Office of Strategic Research told this writer: "What you do not understand is that while we may not always be right, we are never wrong." In a recent book on the CIA one reads "... On the whole, the CIA seems to have done its job well, especially where Soviet military capabilities have been concerned. There have been no major surprises, no sudden discoveries of developments

Table 2
Revised Box Score of Intelligence Forecasts

	Under estimate	Over estimate	Near the Mark
<i>Weapon System Deployments</i>			
Heavy Bombers			X
Medium Bombers	X		
1st Generation ICBMs			X
IR/MRBMs	X		
3rd Generation ICBMs		X	
2nd Generation SLBMs	X		
ABM Deployments			X
Advanced Nuclear-Powered ASW Submarine	X		
<i>Ground and Air Force Modernization in 1970s</i>			
Tanks and Armored Personnel Carriers	X		
Self-Propelled Guns	X		
Operational-Tactical Missiles		X	
Fighters and Fighter-Bombers	X		
Helicopters	X		
Transports	X		
SAMs	X		
<i>Selected Technologies</i>			
1st A-Bomb Test	X		
1st H-Bomb Test	X	X	X
ICBM MIRVs	X	X	
4th Generation ICBMs (MIRVs)			
Accuracy	X		
Yields		X	
SLBMs MIRVs	X		
Operational-Tactical Missile and Aircraft Avionics & Electronics			X
<i>Soviet Defense Expenditures</i>			
1960-1976 Period		X	
1976-1980 Period		X	

Source. The underestimates of the scale and pace of Soviet modernization and deployment programs for the Ground Forces, Frontal Aviation and Military Transport Aviation in the 1970s have been inferred from the pertinent sections of the annual Posture Statements by the various Secretaries of Defense.

in Soviet military technology that might leave the United States in a dramatic disadvantage."³³

It is difficult to reconcile this statement with the record as shown in Table 2. One can grant that perhaps no single one of the series of surprises put the United States at a "sudden" military disadvantage. The cumulative effect of all the underestimates on U.S. arms policies, however, has been substantial.

Some mild surprises are probably unavoidable. A case in point is the recent appearance

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of a large new Soviet nuclear submarine said not to be the expected "Typhoon" SSBN.³⁴ (Incidentally, the rate at which the Soviets are building large, expensive new naval combatants leads one to believe that the only constraint on Admiral Gorshkov's budget may be the champagne that it takes to launch all his new vessels.) In any event, it seems that U.S. intelligence analysts knew that a large Soviet submarine was under development: an SSBN referred to by the Soviets as "Typhoon," which may be larger than Trident and the first boat of which probably will be launched soon. But the new submarine already sighted is not the "Typhoon." Thus the surprise is that there are *two* new classes of very large nuclear-powered submarines rather than just one, both apparently being built in the same Soviet shipyard. That sort of thing is difficult to detect. Yet, the incident should serve to impress upon us the limits of U.S. "national means of verification." The Soviets would not be able to build a fleet of new submarines in secret; nevertheless, the fact that such a large Soviet weapons project could reach this stage before being detected should give everyone pause for reflection on the limitations of external collection sensors.

Given the magnitude and continuity of the Soviet arms build-up since 1958, more surprises probably lie in store for us. There is some intelligence which only human sources (reportedly much downgraded in the past four years) can provide.

This brings us to the related issues of information sources, forecasting methods and the quality of the forecasts. Mr. Aspin dates what may be called the revolution in U.S. "national means of verification" to August 1960: "A U.S. photo-reconnaissance satellite took the first clear pictures of a Soviet ICBM site at Plesetsk in August 1960. . ." The 1960s inaugurated the era when the U.S. could, by non-intrusive technical means, count with some confidence such phenomena as ICBM launchers, aircraft on airfields, naval vessels and eventually a great deal more. Prior to the early 1960s the U.S. did not know how many missiles the Soviets had or had not deployed; from then on we knew. Indeed, it was this revolution in "national means of verification" that made SALT possible in the 1970s, because until at least 1978 U.S. intelligence provided all the data on Soviet weapons systems—the numbers and characteristics—required to negotiate the various SALT agreements.

Also in the early 1960s the Soviets began to publish at great length the rationale for their decisions on weapons system acquisition and their objectives in the arms competition with the United States. As we now know from declassified documents, from 1960 onward the United States had reasonably continuous access

to the journal of the Soviet General Staff, *Military Thought*. Under such circumstances one might have expected the CIA to have improved the quality of its forecasts of Soviet weapons programs.

The record, however, does not show any improvement. In the 1950s when the CIA had very little "hard" intelligence on Soviet deployments and not much intelligence on the Soviet strategic rationale, the national estimates both over- and underestimated Soviet strategic force deployments and technologies to a more or less equal degree. After the revolution in U.S. "national means of verification" and after the Soviet strategic rationale could be gleaned from both unclassified and classified Soviet sources, the CIA might reasonably have been expected to have learned from past mistaken estimates. If anything, however, the record since that time is much worse. Only one Soviet strategic deployment program (ABMs) was overestimated after 1960; all the rest were underestimated. The forecasting of Soviet weapons technologies did improve a bit. Since 1960 four major Soviet weapons technologies have been underestimated and two overestimated.

The CIA's assessments and forecasts of Soviet defense expenditures and national economic priorities have been the most unambiguously dismal failures of all. We have described two gross underestimates, which have come back-to-back and for essentially the same reasons.

As was pointed out earlier, even the expanded list in Table 2 is still a modest sample of the hundred-odd major procurement programs in which the Soviets have engaged in during the past three decades. Noteworthy programs missing from the list are Soviet air defense radars, SAMs and interceptors. The record of the CIA and the other intelligence services in forecasting Soviet programs in these areas may well have been better. Unfortunately, the information available in the public domain is not adequate to assess the forecasting record with respect to these Soviet defensive weapons systems.

Even if the CIA's forecasting record would improve a bit by further expansion of the sample, the general conclusion would remain the same: U.S. intelligence has underestimated Soviet weapons programs far more often than it has overestimated them; and the forecasting record since 1960-1961 seems worse than before, despite the revolution in technical intelligence collection capabilities over the past two decades and the relative abundance of evidence available from classified and unclassified sources on the Soviet rationale for decisions on weapons acquisition.

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The Question of Methodology

What methodology (or methodologies) has the CIA developed to forecast Soviet weapons production and deployment programs and Soviet weapons technologies? Given the record, the methodologies have not worked very well. On the basis of the record, it also is difficult to believe that forecasting methodologies have been (1) consistent from one class of NIE to another—that is, from strategic offensive to strategic defensive to general purpose forces—or (2) consistent among various editions of the same NIE—for example, successive editions of the NIE for Soviet strategic offensive forces.

One glaring deficiency in CIA forecasting methodology for Soviet ICBMs has been provided in the public domain by former CIA analyst David Sullivan, who points out that during the 1960s the CIA ignored the role of the Soviet Five Year Plans in determining future deployment programs.⁸⁵ In effect, CIA analysts assumed that all the ICBM silos known to be under construction each year would be completed, but that no new silos would be started. Hence, every year the NIEs forecast that Soviet ICBM forces would peak when all the silos then under construction were completed. This approach ignored both the five-year planning cycle as well as any type of requirements analysis to determine how many ICBMs, or warheads, the Soviets might ultimately need to carry out their well-publicized targeting strategy.

In the final section of his article, Congressman Aspin offers six reasons for "misjudgments" that "have distorted our vision." Only some short observations are possible here because a full commentary would require at least as much space as already has been used. Suffice it to say that most of these factors listed by Mr. Aspin are not exactly novel ones. None of them is "fatally inevitable," to borrow a Soviet phrase. If intelligence analysts would consciously seek to avoid these error-prone factors, they could significantly upgrade their intelligence forecasts. For example, simply paying more attention to Soviet sources—the last factor on Congressman Aspin's list—could have avoided successive underestimates of Soviet defense expenditures and national priorities.

Better forecasts were possible. Indeed, better forecasts were made—outside the CIA and the intelligence services and even without benefit of access to classified Soviet literature on the Soviet strategic rationale.

SALT II and U.S. Strategic Forecasts

Some concluding comments are in order on Congressman Aspin's linkage of the SALT II Treaty to U.S. strategic forecasts. His main

point is that, under a ratified SALT II Treaty, the U.S. intelligence community could concentrate its efforts on the task of verifying the compliance of the Soviet Union with the agreed-upon limitations on strategic weapons systems. Conversely, he concludes, "Estimates of future Soviet activity are likely to be wider off the mark than they would be under a SALT II Treaty, simply because the reference points provided by the Treaty have been removed."

This linkage of SALT and forecasting does not stand up on three grounds. First, the SALT II Treaty applies to only a small portion of the Soviet military establishment. About 30 percent of Soviet strategic offensive forces are not accountable under the Treaty. Except for ABM systems, strategic defenses are not covered at all, and there is substantial cause to fear that the Soviet Union will abrogate the ABM Treaty in the mid-1980s. U.S. intelligence agencies are charged with forecasting the disposition of some 40 to 50 major Soviet weapons systems that are not covered by SALT, as well as such related issues as how and to what ends the Soviets would employ their armed forces in the event of war, Soviet defense expenditures, the military burden on the Soviet economy, etc. The SALT II Treaty would not help the intelligence community in forecasting the weapons it does not cover, nor do the Treaty's "reference points" promise to contribute much, if anything, to forecasts of Soviet technological developments even for those relatively few strategic systems that are subject to the Treaty's provisions.

Second, there is no assurance that the SALT II Treaty would be of any greater help to the U.S. intelligence community in its strategic forecasts than was provided ostensibly by SALT I. When SALT began, the United States possessed unquestioned superiority in strategic nuclear systems vis-a-vis the Soviet Union. Today, the debate centers not on the fact of U.S. strategic inferiority, but rather on its degree. None of these changes in the military balance was forecast by the CIA a decade ago. What will the SALT II Treaty do for U.S. intelligence forecasts of Soviet military capabilities that differ from the past SALT experience?

To note one example: Already there are press reports that the Soviets have been testing a new reentry vehicle on the SS-18 missile.⁸⁶ U.S. analysts evidently are concerned that this RV may have some sort of terminal guidance system which could give the Soviets much more confidence in their ability to destroy U.S. ICBMs on the ground. Unfortunately, but not surprisingly, analysis of the new RV is being hindered by Soviet telemetry encryption, which is permitted under the SALT II Treaty. It remains to be seen whether this new RV being tested on

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the SS-18, the prime threat to U.S. ICBMs, is compatible with either the spirit or the letter of the SALT II Treaty.

Third, the implied notion that the SALT II Treaty will somehow help the CIA to do better what it has done badly is the brittlest of arguments in favor of ratifying the Treaty. As has been contended above, the poor record of the CIA in divining Soviet strategic objectives and forecasting Soviet strategic capabilities has not been for lack of evidence or "reference points"; rather, the mistakes have issued from the human failure to interpret accurately the ample evidence that has been available. To be sure, strategic forecasting can never become an exact science, and some surprises in Soviet weapons developments and deployments are probably in-

evitable. Saying this, however, does not exonerate the consistently poor record of the CIA and the other intelligence services in forecasting Soviet forces, at least not since the revolution in the "national means of verification" that took place two decades ago.

The solution to the problem is not a treaty with the Soviet Union that might somehow ease the burden of responsibilities on the CIA and the other intelligence services. The solution, rather, lies in such improvements as are necessary to ensure that forecasting is done more competently. Meanwhile, let the fate of the SALT II Treaty ride or fall on the putative claim that it will control strategic arms in ways that contribute to the security of the United States and its allies.

NOTES

1. Robert F. Ellsworth and Kenneth L. Adelman, "Foolish Intelligence," *Foreign Policy*, Fall 1979, pp. 147-159.

2. For a summary of the historical pattern, see William T. Lee, "The Soviet Defense Establishment in the '80s," *Air Force Magazine*, March 1980, pp. 100-108, most specifically, Table 1 on p. 103.

3. I shared this faulty perception of Soviet strategic priorities in the early 1960s and previously have discussed it at some length. William T. Lee, "Understanding the Soviet Military Threat" (New York: National Strategy Information Center, Agenda Paper No. 6, 1977), pp. 27-30.

4. *Ibid.*, pp. 33-39, 42-45.

5. This was the "high force/high technology" estimate which assumed the Soviets would drop the three RV version of the SS-9 and move on to MIRVs, which, as it turned out, the Soviets did. However, it took them until 1974 to do it, the date Congressman Aspin says the 1969 NIE forecast Soviet MIRVs via the "low force/low technology" route.

6. Charles S. Sheldon II, "Soviet Space Programs, 1966-1970," Staff Report prepared for the Senate Committee on Aeronautical and Space Sciences (Washington, D.C.: Government Printing Office, 1971), p. 168. These two shots were the only launches in the "Elektron" series. For an independent evaluation of Soviet programs leading to the same conclusion, see David S. Sullivan, "The Legacy of SALT I: Soviet Deception and U.S. Retreat," *Strategic Review*, Winter 1979, p. 33.

7. The first flight test of the three RV, "triplet" version of the SS-9 ICEM occurred in August 1968, *Statement of Secretary of Defense Melvin R. Laird on the Fiscal Year 1972-76 Defense Program and the 1972 Defense Budget*, (March 9, 1971), p. 46.

8. Secretary of Defense Melvin R. Laird, *Fiscal Year 1971 Defense Program & Budget*, (February 20, 1970), p. 104. This forecast followed the "high force/high technology" route. According to Secretary Laird, the NIE did not offer a "most likely" Soviet course between the "high force/high technology" and "low force/low technology" routes. Furthermore, the confusion is compounded because Secretary Laird's Report is dated February 20, 1970. Therefore the NIE he referenced must have been written in 1969 when Congressman Aspin says the Board of National Estimates ruled out Soviet counterforce capabilities through the 1970s. Secretary Laird said the MIRV forecast would give the Soviets a "formidable hard-target capability by the mid-1970s" while the multiple RV forecast would provide a "considerable" capability in the same period

9. Admiral Thomas H. Moorer, *United States Military Posture for FY 1972*, March 9, 1971, p. 7, Senator Gordon J. Humphrey, "Analysis and Compliance Enforcement in SALT Verification," *International Security Review*, Spring 1980.

10. Laird, *FY 1971 Defense Program & Budget*, p. 8.

11. *Statement of Secretary of Defense Robert S. McNamara, Before a Joint Session of the Senate Armed Services Committee and the Senate Subcommittee on Department of Defense Appropriations in the Fiscal Year 1968-72 Defense Program & 1968 Defense Budget*, (Washington, D.C.: Government Printing Office, January 23, 1969), pp. 43-45.

12. *Ibid.*, p. 53.

13. Congressman Aspin cites a CIA estimate of 0.5 nm CEP for the SS-9 triplet in 1971 and "initial press leaks" (undocumented) in 1973 giving the same CEP for "first tests" of the new Soviet MIRVs. The first documented report available to this author puts the accuracy of Soviet MIRVs at 0.3 nm in the mid-1970s, *Air Force Magazine*, March 1977. Secretary of Defense James R. Schlesinger apparently told Congress in 1975 that Soviet MIRVs were accurate to 0.33 to 0.25 nm. Clarence Robinson, "Cabinet Shifts May Speed SALT," *Aviation Week and Space Technology*, November 10, 1975, p. 12. Later reports, reflecting subsequent testing, put the CEP of the SS-17, SS-18 and SS-19 at 0.1 to 0.06 nm. *Aviation Week and Space Technology*, February 11, 1980, p. 12.

14. For the link between these precision tools and improvements in Soviet MIRV accuracy, see Senator Harry F. Byrd, Jr., commenting on the testimony of Dr. Jack Vorona of the Defense Intelligence Agency before the Subcommittee on General Procurement of the Senate Committee on Armed Services, *Congressional Record*, Senate, November 13, 1979, p. S 16506.

15. For a survey of much of the pertinent Soviet literature, see William T. Lee "Soviet Nuclear Targeting and SALT" in Steven Rosefielde, editor, *World Communism at the Crossroads* (Boston: Martinus Nijhoff, 1980), pp. 55-88.

16. General Major V. Kruchinin, "Contemporary Strategic Theory on the Goals and Missions of Armed Conflict," *Military Thought*, No. 10, 1963.

17. For a first approximation of such calculations, see W.T. Lee "Soviet Nuclear Targeting and SALT," op. cit. Refinement using Soviet instead of U.S. estimates of soft targeting vulnerabilities, damage criteria and objectives now in process reinforce these preliminary calculations.

18. For a first approximation of Soviet strategic missile allocations and reserves against the Conus target array, see W.T. Lee "The Soviet Defense Estab-

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- lishment in the 80's," op. cit., p. 105. (Note that the ordinate in Figure 1 should be in thousands.)
19. Marshal N. Krylov, "The Nuclear Missile Shield of the Soviet State," *Military Thought*, No. 11, 1967, p. 18.
20. R.A. Robinson, "The Evolution of Soviet Naval Policies and Programs (Part 3), Cuba Missile Crisis to the Late 1960s," *American Intelligence Journal*, Spring 1980.
21. Ibid. This happened in mid-July 1966, probably just a few months before the first Y-class SSBN was launched.
22. Secretary of Defense Donald H. Rumsfeld, *Annual Defense Department Report FY 1977*, p. 53.
23. Secretary of Defense Donald H. Rumsfeld, *Annual Defense Department Report FY 1978*, p. 63.
24. "Statement of Data on the Numbers of Strategic Offensive Arms as of the Date of Signature of the Treaty," signed by V. Karpov, Chief of the USSR Delegation to the Strategic Arms Limitation Talks, printed in U.S. Department of State, Bureau of Public Affairs, Selected Documents No. 12A, *SALT II Agreement* (Vienna: June 18, 1979).
25. Jack Anderson, *Washington Post*, December 12, 1976.
26. Captain John H. Moore, ed., *Jane's Fighting Ships 1975-76 & 1979-80* (London: Jane's Yearbooks).
27. Secretary of Defense Harold Brown, *Annual Defense Department Report FY 1981*, p. 103.
28. Admiral A. Chabenko, "Combat Rocket Carrying Atomic Submarines," *Military Thought*, No. 12, 1967, pp. 45-47. Admiral N. Kharlamov, "Some Trends in the Development of Navies," *Military Thought*, No. 10, 1967, pp. 66-67. That such a submarine would have a titanium hull was indicated by Captain N. Shatrov, "Trends in the Development and Employment of Naval Fleets," *Military Thought*, No. 1, 1972, p. 55.
29. Secretary of Defense Brown, *FY 1981 Report*, p. 103.
30. Secretary of Defense Donald H. Rumsfeld, *Annual Defense Department Report FY 1977*, p. 97.
31. Secretary of Defense Brown, *FY 1981 Report*, p. 38.
32. Secretary Rumsfeld, *FY 1977 Report*, p. 81.
33. Thomas Powers, *The Man Who Kept the Secrets. Richard Helms and the CIA* (New York: Alfred A. Knopf, 1979), p. x.
34. "Giant Soviet Submarine Reported," *New York Times*, May 8, 1980. Given the pattern of Soviet nuclear powered submarine developments, both a new model cruise missile SSN and a new model ASW SSN should appear at any time. Given the reported size of this new SSN, it probably is the former rather than the latter. It will be particularly interesting to see if this new submarine also is a titanium hull model designed to realize the tactical advantages of greater depths and high speed as described by Admirals Kharlamov and Chabenko and Captain Shatrov in the *Military Thought* articles previously cited.
35. David S. Sullivan, "Evaluating U.S. Intelligence Estimates," in Roy Godson, ed., *Intelligence Requirements for the 1980s. Analysis and Estimates* (New York: National Strategy Information Center, 1980), p. 54.
36. Henry S. Bradsher, "New Soviet Missile Tests Watched Closely by U.S.," *Washington Star*, June 3, 1980, p. 7.

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A REBUTTAL BY CONGRESSMAN ASPIN

William Lee's criticisms of my article appear to fall into four rough categories: (1) that I did not analyze every intelligence estimate concerning Soviet military matters of the past two decades; (2) that I did not clearly state where I obtained some of my facts; (3) that some things I classify as overestimates were actually underestimates and that I provide misleading alibis for CIA errors; and (4) that SALT does not really provide strategic predictability. In addition, Mr. Lee raises the important point that some misestimates are much more significant than others, a point he claims I neglect.

Mr. Lee's first two criticisms can be dealt with quickly. Basically, he chides me for not including in my article such intelligence estimates as those pertaining to Soviet theater forces and defense budgets. Let me just say that Mr. Lee should have addressed himself to the article I wrote, not to the one he would have written. I state very clearly, up front, that the article is about *strategic* forecasting. I do so chiefly to put the issues of a world without SALT into some perspective. While conventional force estimates and the size of the Soviet defense budget are interesting issues—and they

are certainly on the menu of topics I will be investigating through the House Intelligence Committee—they fall outside the realm of the topic I was addressing in my article. Incidentally, I certainly regard Mr. Lee's comments as valuable indicators of what problems may be worth examining. I should also remark that I find his estimates of the Soviet defense budget—which proved more accurate than those of the CIA prior to 1975—to have been an important contribution.

As to the sources of my facts, this is more difficult terrain. I have access to classified intelligence data as a member of the House Permanent Select Committee on Intelligence. Committee rules forbid me from disclosing classified information received through the Committee. Thus, I used only public statements and other open literature. This constraint shaped the choice of estimates and examples I included in the article. Many of the topics raised by Mr. Lee in his commentary were excluded by me because they are treated inadequately or not at all in public sources. This applies notably to the 1965 National Intelligence Estimate (NIE) and Mr. Lee's comments about what he had learned about its con-

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tents and the internal controversy surrounding it.

Third, there is Mr. Lee's criticism of my classification of certain CIA errors as under- or overestimates. His reworking of my table seriously distorts the matter. First, he misunderstands my point about the role of peripheral systems (IR/MRBMs and medium-range bombers) in the "bomber gap" and "missile gap." He criticizes me for not recognizing that the CIA's underestimate of Soviet medium-range forces was caused by, or at least proportional to, CIA overestimates of intercontinental forces.

Yet, this is precisely what I did say, except in reverse order—i.e., that the overestimate of long-range bombers and ICBMs was caused by lack of appreciation that the Soviets planned to build more forces targeted in the theater. Mr. Lee says, "Mr. Aspin excuses U.S. intelligence services on the grounds that they did not grasp either Soviet strategic priorities or the Soviet perception that the immediate threat to the USSR emanated from NATO and U.S. overseas bases. . . ." (Emphasis added.) Yet, I did not excuse them for this error; I criticized them for it. Indeed, a main point of my article was that CIA errors have often been caused by misjudgment of Soviet priorities and intentions and that this constitutes a highly serious—perhaps the critical—problem. If Mr. Lee does not understand that I was making this point, then I am afraid he has badly misread my article.

He also misread my article when he claims that I misinterpreted estimates of the date when the Soviets would have MIRVed ICBMs. He writes as if I were talking about when the Soviets would *develop* MIRVs, when my article clearly refers to estimates of MIRV *deployment*. Mr. Lee argues, furthermore, that the 1969 NIE's forecast of Soviet MIRV development (1971) was only three years off the mark, which is quite good compared with the 1968 NIE (1978). Actually, since I was talking about estimates of MIRV *deployment*, and since the Soviets deployed MIRVs in 1975, both estimates are equally off the mark—by four years.

Mr. Lee is also misleading on the matter of the Bureau of National Estimates' statement in 1969 that the Soviets "could not, and would not try to" attack Minuteman silos effectively in the 1970s. Mr. Lee considers this a dramatic misjudgment, but it needs to be considered in context. While the Soviets did, indeed, develop some capability of endangering Minuteman missiles in the 1970s, the BNE was talking about "first-strike capability"—i.e., the potential to knock out Minuteman without suffering substantial retaliation. On this matter, the BNE was right in principle, as the Soviets are only now beginning to acquire a capability to knock out almost the whole Minuteman force.

Moreover, Mr. Lee calls this BNE statement "a most damning indictment of the CIA." This

is polemical: surely Mr. Lee knows that the BNE and the CIA are not the same institution. In fact, Mr. Lee's accusation against the CIA for underappreciating the Soviet threat to Minuteman should instead be leveled against the Joint Chiefs of Staff, who convinced Defense Secretary Clark Clifford to delete from a memo to President Johnson a warning based on (as it turned out, false) intelligence that the SS-9 might pose a threat to Minuteman. (Incidentally, Mr. Lee repeats the old error of referring to the SS-9 "triplet" as a MIRV—a tendentious and misleading epithet, as my article points out.)

Mr. Lee is also misleading, or perhaps simply confusing, when he challenges my claim that the intelligence community overestimated the yield of SS-18 RVs on the grounds that I need "to demonstrate that the figures of 1.5 megatons [the overestimate] and .6 megatons [the revised and now-believed correct estimate] refer to one and the same RV model." This is illogical. If intelligence community evaluations of explosive yield refer to deployed RVs, and if these RVs are in fact .6 megatons, then a forecast that RVs carrying 1.5 megatons would be deployed is much too high, whether or not the two RVs are the same model.

A basic problem in Mr. Lee's rebuttal is that he simply misses the point of my article. I attempted to address the perception that the intelligence community is biased toward overestimating or underestimating Soviet strategic programs. My conclusion is that the community does both. This conclusion is insensitive to the coincidence that the cases I examined were divided about equally between overestimates and underestimates. The point would be equally valid if the division were not so equal. (If, as Mr. Lee claims, the division were lopsided in one direction, we would have a serious problem, but for strategic estimates it is not lopsided.) The *important conclusion for policymakers* is that intelligence forecasts cannot be as reliable as we would like them to be, and therefore a major contribution of arms control treaties such as SALT is that they introduce a measure of certainty (or at least bounded expectations) into an otherwise uncertain environment.

Mr. Lee's criticisms of this conclusion concerning SALT distort the argument. Mr. Lee first observes that SALT II does not cover all Soviet systems. But SALT does cover the systems of most importance to the United States. The ability to know the limits of Soviet options for the systems covered by SALT is a plus, even if the other systems remain difficult to predict.

Second, Mr. Lee claims that SALT I did not enable the intelligence community to forecast the onset of U.S. strategic inferiority. This of course is not the intelligence community's job.

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Any forecast of the strategic balance requires prediction of American as well as Soviet forces. Prediction of American forces is improper for intelligence. In recent years the intelligence community has begun comparing its *predictions* of Soviet forces to *plans* for U.S. forces. These so-called "net assessments" are a valid intelligence function, but they do not permit forecasts of the strategic balance except in the conditional sense: if the United States carries out its plans.

As Mr. Lee well knows, furthermore, doomsayers in the intelligence community went well beyond their mandate by forecasting U.S. inferiority in the early 1980s. Along with elements of the intelligence community, I have challenged the claim that these forebodings have proven correct.

Third, Mr. Lee asserts that the "implied claim" that SALT will help the intelligence community forecast Soviet strategic forces is the "brittlest" of all arguments in favor of ratification. While SALT II does ease the intelligence agencies' forecasting task, this is a side issue. The benefit of SALT is that it offers to policymakers a more predictable strategic outlook. Judged by the record, the intelligence community cannot supply this predictability, which is useful for rational choices about future forces and diplomatic policy.

Mr. Lee notes that some misestimates by the U.S. intelligence community are more serious than others. I agree with the conclusion, but differ somewhat on the criterion. Misestimates are serious when they cause us to make choices different from those we would have made if faced with a correct estimate, and when these poorly based choices place us in a disadvantageous position. Few intelligence estimates meet this criterion of significance. Many factors totally unrelated to intelligence enter into policymaking, and it is rare that these other factors are so evenly balanced that intelligence can be shown to have made the decisive difference. However, on occasion it has made a difference, and that difference has been caused by overestimates as well as by underestimates.

For example, the intelligence community's overestimates of Soviet MIRV and ABM capa-

bilities almost certainly played a role in the Senate's 1969 approval, by one vote, of deploying an American ABM system. Had the intelligence community realized then, as it did two years later, that the SS-9 "triplet" could not threaten Minuteman and that the Soviets would not try to deploy a nationwide ABM, one Senator might very well have voted "no" instead of "yes," and we would have been spared the waste of deploying an inadequate, unworkable ABM that we later dismantled. Mr. Lee seems to think that overestimating Soviet capabilities is more acceptable than underestimating them. However, this example indicates that whether one kind of error is more harmful than the other depends on the specific details of the case.

The main point of my article is that intelligence errors are caused more by misunderstanding of Soviet priorities and requirements and by imperfect analysis than by inadequate technical collection. I infer that Mr. Lee generally agrees with this. I concur with his notion that we must have an intelligence community able to admit its own errors. I can also comment that whatever the failings of the past, the House Intelligence Committee has not found intelligence officials unwilling to admit where they have gone wrong once it can be demonstrated that something is wrong.

I should add that, contrary to Mr. Lee's suggestions in his final remarks, simply because individuals can make predictions that occasionally turn out correct, this does not mean that all equally objective and knowledgeable observers would reach the same views ahead of time. In intelligence there are always differing opinions, even when all the analysts are unbiased. There is also the problem of political interference, some cases of which are discussed in my article. It is against improper political interference—and not all political actions which influence intelligence are improper—that we must take aim. We need to encourage vigorous, technically competent research and analysis within the intelligence community; and we need to make policy in recognition that crystal-ball gazing has inherent limitations when it comes to forecasting Soviet strategic forces.

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95th Congress }
2d Session }

COMMITTEE PRINT

THE NATIONAL INTELLIGENCE ESTIMATES A-B
TEAM EPISODE CONCERNING SOVIET STRATE-
GIC CAPABILITY AND OBJECTIVES

REPORT

OF THE

SENATE SELECT COMMITTEE
ON INTELLIGENCE
SUBCOMMITTEE ON
COLLECTION, PRODUCTION, AND QUALITY
UNITED STATES SENATE
TOGETHER WITH
SEPARATE VIEWS



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THE NATIONAL INTELLIGENCE ESTIMATES—A-B TEAM EPISODE CONCERNING SOVIET STRATEGIC CAPABILITY AND OBJECTIVES

The Senate Select Committee on Intelligence, as part of its oversight function, has conducted a study of the 1976 "A Team-B Team" experiment in comparative assessments of Soviet strategic strength which was initiated by the President's Foreign Intelligence Advisory Board (PFIAB). The committee conducted this inquiry under its mandate to evaluate the collection, production, and quality of U.S. intelligence, in this case assessing whether the A-B experiment had proved to be a useful procedure in improving National Intelligence Estimates (NIE's) on a centrally important question.

The pertinent facts of the A-B case are (a) that PFIAB commissioned three ad hoc outside groups (composing the "B Team") to examine the data available to the U.S. intelligence community's analysts (the "A Team"), to determine whether such data would support conclusions on Soviet strategic capabilities and objectives different from those presented in the community's NIE's; and (b) that during the exercise details of these sensitive questions leaked on several occasions to the press.

The committee has prepared a classified report on the subject, sent copies of that report to the executive branch, made copies available to certain members of the B Team for review and comment, and subsequently rechecked the record thoroughly and accommodated some of the B Team members' comments. A summary of the committee's report follows.

SCOPE OF THE COMMITTEE INQUIRY

The committee sought to determine the facts and issues central to the A Team-B Team case, and to give a critique of the procedures which underlay the principal judgments and conduct of both the A and B Teams. The committee's report makes no attempt to judge which group's estimates concerning the U.S.S.R. are correct. The report focuses on the processes followed; its findings and recommendations for improving the quality and utility of future NIE's on Soviet strategic capabilities and objectives are primarily directed at procedural issues.

THE FACTS OF THE CASE

In the broadest sense, the NIE-B Team episode derived from a growing concern over the U.S.S.R.'s steady increase in strategic weapons strength over the course of the past decade and disagree-

ment within the U.S. intelligence community¹ on the meaning of this growth.

The B Team experiment in competitive analysis stemmed from the PFIAB's opinion that the NIE's had been underestimating the progress of Soviet strategic weapons.² In an August 1975 letter to President Ford, PFIAB Chairman George W. Anderson, Jr., proposed that the President authorize the NSC to implement a "competitive analysis." The then Director of Central Intelligence (DCI) William E. Colby, speaking with the unanimous agreement of the U.S. Intelligence Board (the chiefs of the intelligence community components), responded with a proposal that the PFIAB first examine an applicable NIE then underway and thereafter determine what specific course of action to take.

The PFIAB found weaknesses in that NIE and, after having made further investigations of its own, again proposed³ (in April 1976) an experiment in "competitive analysis." The PFIAB recommended that the exercise be placed under the DCI's jurisdiction and that it address certain critical estimative issues.

PRINCIPAL JUDGMENTS AND RECOMMENDATIONS

The committee's report includes these central judgments:

That the concept of a review of the NIE's by outside experts was a legitimate one.

That the B Team made some valid criticisms of the NIE's, especially concerning certain technical intelligence questions, and some useful recommendations concerning the estimative process, but those contributions were less valuable than they might have been because (1) the exercise had been so structured by the PFIAB and the Director of Central Intelligence (DCI) that the B Team on Soviet objectives reflected the views of only one segment of the spectrum of opinion; and (2) that Team spent much of its effort on criticizing much earlier NIE's rather than, as had been earlier agreed upon by the PFIAB and the DCI, producing alternative estimates from certain of those of the 1976 NIE.

That the value of the A-B experiment was further lessened by the fact that details concerning these highly classified questions leaked to the press, where these appeared in garbled and one-sided form. It has not been determined who was responsible for the leaks.

That, most importantly, NIE's on Soviet strategic capabilities and objectives still need improvement in a number of important respects.

¹ In the past, the U.S. intelligence community included the Central Intelligence Agency, the Defense Intelligence Agency, the National Security Agency, and the intelligence components of the State Department, Army, Navy, Air Force, FBI, Energy Resources Development Administration, and Treasury.

² As of August 1975, the PFIAB's members, in addition to Chairman George W. Anderson, Jr. (Adm. USN, Ret.) were William O. Baker, Leo Cherne, John S. Foster, Jr., Robert W. Galvin, Gordon Gray, Edwin H. Land, Clare Booth Luce, George P. Shultz, and Edward Teller. As of mid-1976, Mr. Cherne had become chairman, and those additional members had joined the PFIAB: John B. Connally, Gen. Lyman L. Lemnitzer, Robert D. Murphy, and Edward Bennett Williams. The PFIAB function has since been abolished by President Carter.

³ Through its Committee on NIE Evaluation (Messrs. Robert Galvin, Edward Teller, and John Foster).

The report's principal recommendations include:

That a collegial estimative group be formed in place of individual National Intelligence Officers.

That outside critiques of NIE's should continue to be conducted, but should, in each instance, be made by expert groups which are broadly representative in character, and whose procedures are thereafter more strictly monitored by the commissioning authorities than obtained in the A-B case.

The committee's investigation was based upon study of primary documents; examination of the NIE record since 1959 on Soviet strategic weapons developments; and interviews with principals from the A and B Teams, the intelligence community, and the PFIAB. The committee has enjoyed the full cooperation of all the above parties. The comments of DCI Stansfield Turner on the report and the present statement and the views of certain members of the B Team on Soviet Objectives have been given consideration by the committee.

Responding to the PFIAB initiatives, the new DCI, Mr. George Bush, consented to the experiment, and by June 1976, the PFIAB and the DCI had worked out ground rules for a competitive assessment experiment. The DCI, through his representatives, made arrangements for, and monitored the experiment in accordance with, those ground rules. Members of the PFIAB were called upon to assist in the formation of the three B Teams and took an active role in the selection of team members.

The exercise did not simply pit an A (or NIE) team against a B Team. There were three B Teams: two on technical questions and one on Soviet objectives. As for the A side, an NIE on Soviet strategic weapons had already been regularly scheduled earlier in the year, and work on it by the intelligence community had already begun before the B Teams came into being. This NIE was much broader in scope than the particular estimative questions the B Teams had been commissioned to address, and the individual civilian and military analysts involved in producing that NIE represented a wide range of views held within the departments and agencies of the intelligence community on the NIE's many questions.

The NIE participants and the B Teams proceeded to produce their two sets of studies independently, with only occasional direct contact during the drafting phase. After the initial drafts of the three B teams were completed, the two sides confronted one another formally on three occasions. Once the decision to proceed with the exercise had been made, procedural cooperation was good between the intelligence community and the three respective B teams. The specific results differed, however, in the three cases. Those concerning technical questions were the most rewarding: there was a mutual give-and-take, and these B Teams clearly made a constructive contribution. By contrast, the discussions concerning Soviet objectives were more controversial and less conclusive. The B Team on Soviet Objectives contributed some useful critiques concerning certain technical intelligence questions, but there was not much give-and-take on broader issues. The view cited in a December 1976 press article⁴ that the B Team challenge turned the NIE "around 180 degrees" is incorrect.

⁴ New York Times, Dec. 26, 1976.

CRITIQUE

It is the view of the committee that past NIE's could have profited from drawing on experts on Soviet strategic questions from outside the intelligence community, both in and out of Government, and from subjecting NIE analyses and judgments on this and other areas to competing assessments from such sources. The PFIAB's 1975-76 proposition that outside expertise should be used to criticize and evaluate the NIE's was a legitimate one. The exercise in practice, however, fell short of the initial conception.

The composition of the B Team dealing with Soviet objectives was so structured that the outcome of the exercise was predetermined and the experiment's contribution lessened. The procedures followed by the intelligence community in the A-B episode also weakened the overall effort to some degree. The intelligence agencies were cast inaccurately in the role of "doves," when they in fact represented a broad spectrum of views. They needlessly allowed analytic mismatches, by sending relatively junior specialists into the debating arena against prestigious and articulate B Team authorities. And the monitoring of the procedures of the B Team on Soviet Objectives was subsequently fairly loose.

The B Team contributions and the 1976 NIE can also be faulted on various substantive grounds. Because of its narrowly specified purpose and scope, influenced strongly in recent years by the preferences of senior policymaking readers regarding format, the NIE did not address the question of how Soviet strategic weapons development fits into important larger concerns [the entire panoply] of Soviet domestic, military, diplomatic, economic, and cultural efforts. As a consequence, the NIE's discussion of Soviet objectives was too brief to be useful. In the view of some readers, its discussion of Soviet military hardware in certain respects was inadequate to be helpful to high-level officials.

A weakness in both the NIE and the B Team report is their lack of expressed sensitivity to the fact and the significance of world developments other than those directly related to the U.S.-Soviet arms race. The strategic weapons balance is the chief subject of both documents, but both documents nonetheless are dominated by military hardware questions and define "strategic power" quite narrowly. By design, in neither the NIE nor the B report are U.S.-Soviet strategic matters set within the wider framework of other dynamic world forces, many of which are essentially the creatures of neither U.S. nor Soviet initiative or control.

COMMITTEE FINDINGS

Estimates should, of course, be written in an accurate and dispassionate manner. They should reflect the best and most broadly representative expert knowledge possible, from both inside and outside the Government. The sensitive estimative questions at hand should not be argued in the press. These requirements did not obtain in the case of the NIE-B Team exercise.

The field of strategic weaponry is complex, and there is much valuable expertise on the subject outside of the intelligence community. The quality of NIE's on these subjects would benefit from more extensive use of this outside knowledge than is now the case. In this respect, the PFIAB initiative was justifiable and desirable.

To be of maximum value, however, such efforts must employ the best and most competent expertise available. Panels representing only one perspective, whether "hard" or "soft," are not desirable. In this respect, the B Team "experiment" was not as constructive as it could have been concerning Soviet objectives.

The exercise in competitive analysis was devalued by the fact that, contrary to the expressed directions of both DCI George Bush and PFIAB Chairman Leo Cherne, word of these sensitive matters leaked to the press, where it appeared in garbled form.

The A-B Team experience sharply demonstrated the intense preoccupation of the CIA, the rest of the intelligence community, the PFIAB, and policymakers with Soviet strategic weapons and their consequences. This subject is of enormous significance to U.S. policymaking, but there are also other significant questions. The greatest intelligence attention often is given to the least likely Soviet actions, nuclear attack, rather than to Soviet intentions and assertive world activity short of those extremes.

Of most significance, the A-B Team case has demonstrated: (a) that the key question of Soviet strategic intentions and conduct is one which demands the best possible marshalling of U.S. intelligence resources and American brainpower; and (b) that the estimative process needs improvement in this area of concern.

The committee's recommendations for improving National Intelligence Estimates concerning Soviet strategic weapons capabilities and objectives included these judgments:

The intelligence community must more effectively meet the particular needs of particular policymakers. Creative use should be made of other estimative formats, in addition to the current categories of NIE's, tailored to the particular needs, but not the views, of different policymaking entities and levels.

There is need for competitive and alternative analyses. Both within the estimative body and with respect to outside expertise, competing and on occasion alternative estimates should be encouraged. To be fully useful, such initiatives must avoid panels with narrow preconceptions, of whatever kind, to assure the balance necessary for the competitors to evaluate evidence which is often both conflicting and ambiguous.

Estimates must openly express differences of judgment, and clearly indicate the assumptions, the evidence, and the reasoning which produce alternative readings.

Estimates should highlight significant changes from related past estimates, including changing probabilities, the emergence of new important alternatives, and findings that make past estimates false or less relevant.

NIE's should define "strategic matters" more comprehensively than has obtained in recent years, so that Soviet military developments can be better seen within the context of Soviet interests and policies, and in interaction with U.S. and world developments. Enchantment with the details of military hardware must not permit either the producers or the policymaking consumers of intelligence to become deflected from pursuit of the most important estimative questions at hand, those of intentions.

Reliable net assessments are needed to complete an effective estimative process, so that policymakers can better appreciate Soviet strengths and weaknesses by having systematically compared them with those of the United States—a function which the NIE's are not designed to perform. The NSC should commission such net assessments, to be prepared by experts at the national level, including some from the intelligence community.

Policymakers must define the questions, not the answers. The DCI and the intelligence community's estimative body must remain independent in judgment. Judgments must not be bent or suppressed by outside pressures or fear of an uncongenial reaction.